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DISCUSSIONS

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AMERICAN SOCIETY OF CIVIL ENGINEERS

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PAPERS

DESIGN OF HINGES AND ARTICULATIONS IN REINFORCED CONCRETE STRUCTURES

By George C. Ernst,[1] Assoc. M. Am. Soc. C. E.

Synopsis

Information on the design of reinforced concrete articulations is not readily available for convenient review and use by the designer. The purpose of this paper is (*a*) to provide a better understanding of the action of such joints when subjected to thrust, shear, and rotation, by a review of the most important features of past tests, and (*b*) to submit methods of design that will yield appropriate sections.

Introduction

The desire on the part of many designing engineers to provide a relative freedom of rotation at predetermined points in reinforced concrete structures has brought into use the so-called semi-articulated, or reinforced concrete, hinge illustrated in Fig. 1. They may be permanent or temporary depending upon

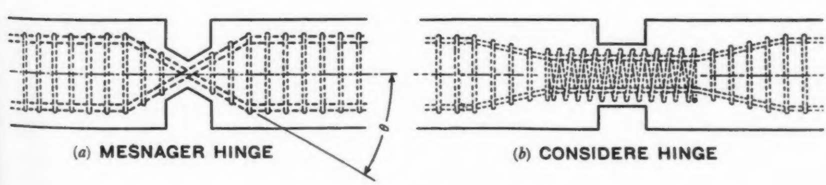

Fig. 1.—Types of Hinges

whether the structure is to be statically determinate throughout its life or only during the period of rapid volumetric change caused by plastic flow, shrinkage, rib-shortening, and the early temperature changes. In either case, it has been found expedient to make the hinge of reinforced concrete, as an integral part of the structure.

Because design methods based upon theoretical analysis, supported by laboratory tests, have not been available, hinges have been constructed fre-

Note.—Written comments are invited for immediate publication; to insure publication the last discussion should be submitted by **August 15, 1940**.

[1] Asst. Prof., Civ. Eng., Univ. of Maryland, College Park, Md.

quently which were not free to rotate properly. Furthermore, reinforced concrete hinges often have been specified because of their novelty rather than their need. Their use is frequently questionable when a high shear is known to exist unless they are detailed with the center line of the hinge virtually collinear with the line of thrust. A review of test results supplemented by design methods and charts for the practical use of the two principal types (see Fig. 1) should supply a long-felt need.

Notation.—The letter symbols used in this paper are defined where they are first introduced and assembled for reference in Appendix I.

TESTS

Mesnager Hinges.—The original tests of A. Mesnager (1)[2] consisted of two specimens subjected to thrust and rotation. They showed, definitely, the feasibility of such construction. The two hinges consisted of sixteen bars 0.79 in. in diameter making an angle of 22° 30′ with the center line (yield point = 42,600 lb per sq in.). The hinges were encased in mortar, and the slenderness ratio $\frac{l}{r}$ of the bar was equal to 21. One specimen was rotated 0.02 radian by means of a steel wedge at one end and then loaded to failure in the rotated position. The ultimate load was 323,000 lb. The other specimen received the same relative rotation (0.02 radian) by an application of wedges at each end. A load of 154,000 lb was applied and released twice prior to reversing the wedges and repeating the loading twice again. The mortar covering was then removed from the hinge and the procedure was repeated prior to loading to failure under a rotation of 0.02 radian. The ultimate load was 238,000 lb. Failure in both specimens resulted from insufficient lateral reinforcement in the end blocks near the hinge opening.

Tests upon seven hinges were reported in 1935 by D. E. Parsons, M. Am. Soc. C. E., and A. H. Stang (2) of the National Bureau of Standards. Three of the hinges contained six $\frac{5}{8}$-in. bars with a slenderness ratio, $\frac{l}{r}$, of 32.4, and the remainder contained eight $\frac{1}{2}$-in. bars with $\frac{l}{r}$ equal to 39.6. The yield strength of the $\frac{5}{8}$-in. bars was 43,600 lb per sq in., and that of the $\frac{1}{2}$-in. bars was 46,200 lb per sq in. Four of the specimens had mortar covering at the hinge and the bars of all specimens made an angle of 30° with the center line of the hinge. Lateral reinforcement consisting of $\frac{1}{4}$-in. stirrups at 3-in. centers was placed in an attempt to prevent cracking of the end blocks. All except two, however, developed such cracks. These two had eight $\frac{1}{2}$-in. bars without mortar covering and one was subjected to a shearing load of 0.292 times the thrust. Four of the specimens which were subjected to direct thrust received total rotations amounting to values from 0.011 and 0.026 radians by means of wedges at each end. In general, the results indicated that the bare bars of the Mesnager hinge could develop 90% of their yield point in direct thrust under the conditions of the tests cited. The work of Messrs. Parsons and Stang was outstanding because for the first time the testing of the combined effect of

[2] Numerals in parentheses, thus (1), refer to the corresponding number in the Bibliography, Appendix II.

thrust and shear was investigated and because they developed formulas for the internal stresses in the bare hinge bars and the transverse reinforcement in the end blocks. The tests of Mesnager and the Bureau of Standards were

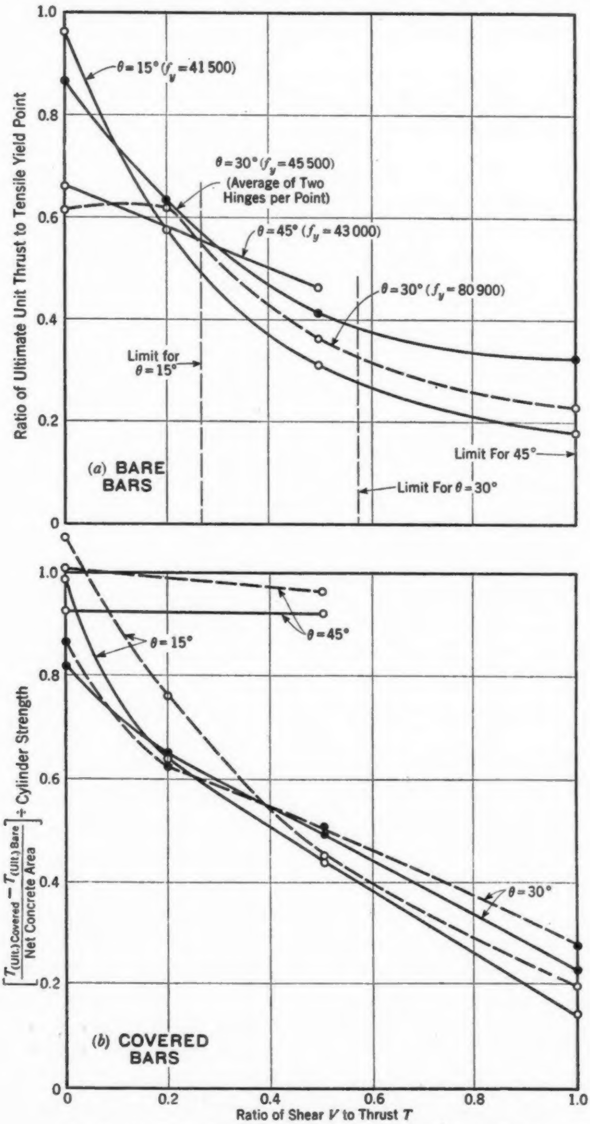

FIG. 2.—DEVELOPMENT OF THRUST ON MESNAGER HINGES (SLENDERNESS RATIO, $\frac{l}{r}$ = 40; AND f_3 = YIELD POINT)

discussed thoroughly in 1935 by Ben Moreell, M. Am. Soc. C. E. (3), who recommended a rational design procedure at the same time.

In 1937 and 1938 tests were made upon Mesnager hinges at the Engineering Experiment Station of the University of Maryland, College Park, Md., for the purpose of checking, completely, the recommended method of design. These tests on 86 hinges (62 of which were reported in 1937 (4)) were the first to combine the effects of thrust, shear, and moment at ultimate and design loads. The investigation of the high shear-thrust ratios likely to be encountered in rigid frame design was also included for the first time in such tests. The Maryland tests, although on somewhat smaller specimens ($\frac{1}{4}$, $\frac{3}{8}$, and $\frac{1}{2}$-in. bars), corroborated those of the Bureau of Standards and substantiated the design recommendations of Admiral Moreell.

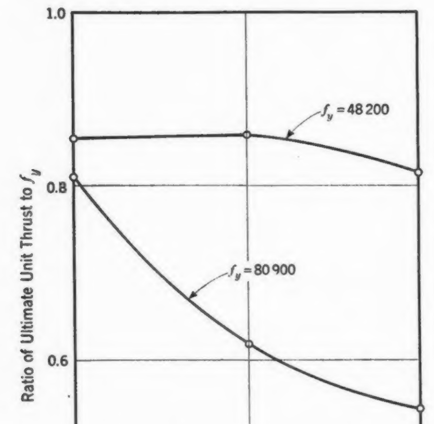

FIG. 3.—SLENDERNESS RATIO VERSUS DEVELOPMENT OF THRUST AND CYLINDER STRENGTH ON MESNAGER HINGES

Figs. 2 and 3, showing the essential data from the Maryland tests in which failure occurred in the hinge, lend emphasis to several new features which are of primary importance. In Fig. 2(*a*) the vertical broken lines are the limits for various values of θ; for example, at $\frac{V}{T} > 0.268$ the load passes outside the bars for $\theta = 15°$ (θ = the angle between the sloping bar and the center line of a Mesnager hinge, in degrees; and V and P = total shear and thrust components of the load P). In Fig. 2(*b*) the solid curves are based entirely upon test results. The broken lines in Fig. 2(*b*) are for an ultimate value of T with the hinge covered (as determined by test) and with the hinge bare (from Eq. 1, presented subsequently). In these tests the concrete strengths were: 2,580 lb per sq in. for $\theta = 30°$ and 1,910 lb per sq in. for $\theta = 15°$ and 45°. At about 30% of the ultimate load a rotation of approximately 0.01 radian, in both directions, was applied and released, the maximum eccentricity of load being 1.06 in. for $\theta = 30°$ and $\frac{V}{T} = 0$. In Eq. 1—the formula for maximum stress presented by Messrs. Parsons and Stang (2) and reviewed by Admiral Moreell (3)—the maximum stresses may be lowered by increasing the slenderness ratio since

the bars are thereby made more flexible. Fig. 3 shows the danger of increasing the value of the slenderness ratio beyond 40 for intermediate grades of steel or beyond 20 for steels having a higher yield point when the bars are not covered. The $\frac{l}{r}$-values of the Mesnager tests were 21 and those in the Parsons-Stang tests were 32.4 and 39.6.

A covering will stiffen the bars materially, as may be seen in Fig. 3(*b*), since the load allotted to the concrete is increased at $\frac{l}{r} = 60$ while at the same time the bare bars show a reduction of load. If it is recognized that the unit functions in a manner similar to that of a reinforced concrete column when the bars are covered, the question of buckling is seen to be of small importance. This same stiffening effect may be noted by comparing the 45° θ-lines in Figs. 2(*a*) and 2(*b*). The computed strength developed in the concrete under direct thrust is higher than when $\theta = 30°$; and, for the ratio $\frac{V}{T} = 0.5$, it is higher for $\theta = 45°$ than for $\theta = 15°$ or $\theta = 30°$. A major function of the covering in the Mesnager hinge is to prevent buckling.

The rapid reduction in the capacity of bare or covered bar hinges, when subjected to high shear-thrust ratios, should sound a warning against their use in the current type of rigid-frame structure unless the design of the hinge is followed rigorously in construction. W. M. Wilson, M. Am. Soc. C. E., and R. W. Kluge, Assoc. M. Am. Soc. C. E. (5), and A. J. Boase, M. Am. Soc. C. E. (6), have shown that, in the usual rigid frame bridge, such hinges are not required as a structural safeguard. Reports of Mesnager hinges in Germany (7) give evidence of a failure under shearing load, either in the hinge or adjoining footing. Tests and design equations point to $\theta = 45°$ for use under high shear-thrust ratios when such construction is deemed essential. If the line of action of the resultant is kept within the extreme hinge bars, as recommended by Admiral Moreell, the upper limit of $\frac{V}{T}$ is 0.268, 0.577, and 1.0 for values of θ equal to 15°, 30°, and 45°, respectively (see Fig. 2(*a*)). It has been shown further (3) that there is an optimum value of θ, for a given value of $\frac{V}{T}$, for which the maximum stresses are lower than for all other values of θ. Considering this optimum, the limits of $\frac{V}{T}$ for $\theta = 15°$, 30°, and 45° become 0.024, 0.195, and 1.0, respectively. In general, the curves of Fig. 2(*a*) indicate that it would be best not to exceed the $\frac{V}{T}$-ratio for which a given θ is the optimum value. The hinge unit may be sloped sufficiently to meet this requirement, if necessary.

For any values of θ and $\frac{V}{T}$, the concrete develops at least $0.8\left(1 - \frac{V}{T}\right)$ of the cylinder strength. A factor of safety of 2.5 would allow for $0.32 f_c' \left(1 - \frac{V}{T}\right)$ to be taken by the minimum concrete section in the design of hinges with

mortar covering, although recommendations for design have previously neglected the covering. This assumes a straight-line reduction from 0.32 at $\frac{V}{T} = 0$ to 0 at $\frac{V}{T} = 1.0$ in Fig. 2(*b*), for all values of θ.

In addition to the foregoing, all tests have shown rotation to have a negligible effect upon the ultimate capacity of hinges with bare bars and a marked effect upon those with covered bars, although a substantial increase over bare bars is still apparent. The capacities of hinges subjected to rotations applied and released prior to the ultimate have not been compared with those of unrotated specimens. It is apparent from Fig. 2(*b*), however, that a substantial resistance still can be supplied by the concrete under the former conditions. The resistance to rotation of either bare or covered bars is negligible.

It has been shown also (1) (2) (4) that a sufficient quantity of transverse steel must be placed close to the hinge opening and wired or welded in intimate contact with the longitudinal steel in order to prevent splitting of the adjoining members by the transverse components from the bars. Eq. 4, given subsequently, is an approximate formula for the determination of the quantity and position of the transverse steel, proposed by Messrs. Parsons and Stang. It gives satisfactory values in comparison with results from Parsons-Stang tests when the effective length for tensile resistance along the axis of the hinge is taken at not more than eight times the diameter of the longitudinal bars. The tests at the University of Maryland were conducted on considerably shorter specimens than those of the Bureau of Standards and the end blocks did not provide adequate embedment for bond in the case of the bars that were ½ in. in diameter. However, the formula for transverse reinforcement provided reasonable values for all tests in which the embedment for bond was not less than 22 diameters. For hinges with covered bars, the total shear V is taken by the sloping bars, as has been demonstrated by T. D. Mylrea, M. Am. Soc. C. E. (8), in the case of wedge shaped beams, but the opposing transverse components in the bars from the direct total thrust should be taken care of by web reinforcement.

Considère Hinges.—The Considère hinge is essentially a short section of spiral column, its dimensions being such that the resistance to rotation is small compared to that of the main adjoining members (see Fig. 1(*b*)).

In 1931 a synopsis of the results from several tests made in 1928 upon Considère hinges was published by C. B. McCullough, M. Am. Soc. C. E., and E. S. Thayer (9). These tests indicated the suitability of the Considère hinge for heavy duty service but did not supply any information upon the resistance of such construction to combined shear, thrust, and moment. Messrs. McCullough and Thayer stated, however, that the Grants Pass arch bridge, in Oregon, in which the Considère hinge was utilized, showed a vertical crown settlement in close agreement with the computed value when the arch was swung, which would indicate little angular restraint resulting from the stiffness of this type of hinge. Successful use of the Considère hinge on the Fourth Street Viaduct in Los Angeles, Calif. (10), was also reported by Leon Blog.

Additional tests were made at the University of Maryland in 1938 (11) upon sixteen Considère hinges using two concrete strengths, three spiral percentages, and shear-thrust ratios of 0, 0.2, 0.5, and 1.0. These hinges were subjected to rotations of as much as 0.018 radian approximately at design loads and in

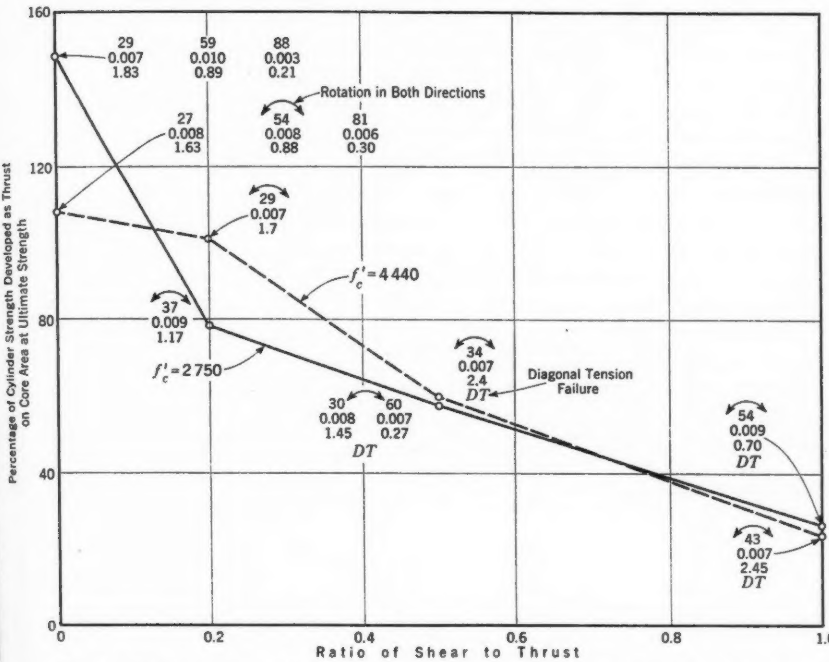

FIG. 4.—DEVELOPMENT OF CYLINDER STRENGTH ON CORE AREA OF CONSIDÈRE HINGES

some cases well in excess of design loads. Figs. 4 and 5 provide a condensation of the test results for study and comparison with Mesnager hinges. The sets of three numerals at each plotted point signify, respectively: The percentage of the ultimate load at which rotation was applied; the maximum change in rotation, in radians; and the maximum eccentricity of the thrust, in inches. The test specimens were 5 in. by 5 in. in cross section, with a 4-in. hinge opening. The spiral reinforcement was a 10-gage steel rod bent to a 4-in. diameter at a 1-in. pitch ($p = 1.43\%$); and the longitudinal reinforcement comprised four $\frac{3}{8}$-in. round rods ($f_y = 48{,}200$ lb per sq in.).

The tests of Messrs. McCullough and Thayer show a development of 200% or more of the cylinder strength under direct thrust for spiral steel percentages of 3.25 and 6.50 (f_c' = ultimate strength of plain concrete = 6,000 to 6,400 at 60 to 90 days). This same effect is to be noted in Fig. 5 for hinges under short time loading and subjected to no rotation; but it is also extremely important to note that the spiral is ineffective in providing such a marked increase following rotations up to 0.011 radian. Rotations below about 30% of the ultimate load, for either grade of concrete, appear to be somewhat less damaging

than those applied up to 88% of the ultimate load. In any case, however, it would be difficult to justify the use of more than 100% of the cylinder strength as the ultimate capacity for such construction under working conditions that do not include shear.

Fig. 4 shows essentially the same marked reduction in development of cylinder strength under shear as exists for the Mesnager hinge (Fig. 2(*b*)). The resistance to rotation as indicated by the maximum eccentricity of thrust of 2.45 in. would certainly be negligible in most cases.

It has been demonstrated (11) that the action of the Considère hinge is essentially that of a spiral column subjected to thrust and shear with a superimposed rotation that places the concrete in the plastic region on the high compression side. Such action suggests the limitation of deformation rather than stress during rotation, with preliminary proportioning for thrust the same as for a spiral column. The spiral may be utilized for protection against diagonal tension failures in the manner that is customary for members subjected to thrust and shear.

Considère Rollers.—Frequently, it is desirable to provide a connection which has no resistance to moment or shear. Although such construction generally has taken the form of a steel rocker or roller nest, it is entirely feasible to use reinforced concrete. The use of the Considère roller in reinforced concrete has been described by W. L. Scott (12) who also provides design formulas and procedure. Design methods for a variation of this type of unit are given by

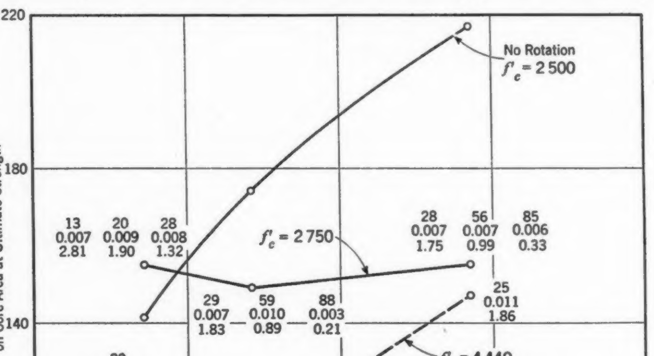

FIG. 5.—QUANTITY OF SPIRAL STEEL VERSUS DEVELOPMENT OF CYLINDER STRENGTH ON CORE AREA OF CONSIDÈRE HINGES $\left(\frac{V}{T} = 0\right)$

Sanford E. Thompson, M. Am. Soc. C. E., and the late Frederick W. Taylor, and E. Smulski (13).

The test roller in Fig. 6 had no longitudinal reinforcement but a 10-gage spiral bent to 5-in. or 6-in. diameter, at a 1-in. pitch. In addition, grid stirrups

of $\frac{3}{16}$-in. round rods were placed at the top and bottom of the spiral and in each bearing block so as to be approximately 0.5 in. from the contact faces. Twenty-two such rollers were tested with the Considère hinges described herein. Compressive strength was varied from 990 lb per sq in. to 4,940 lb per sq in. with

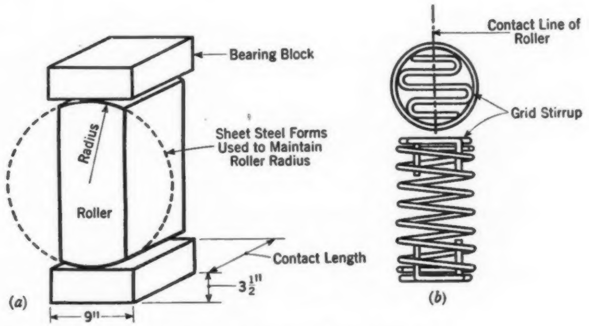

FIG. 6.—CONSIDÈRE TEST ROLLER

roller diameters of 9 in., 12 in., 15 in., and 18 in., spiral steel percentages of 1.15 and 0.95, inclinations of load with center line of spiral of 0, 0.1, and 0.2 radians, and contact lengths of 7 in., 11 in., and 15 in. Deformations were measured to include those occurring in roller and concrete bearing blocks of the same mixture.

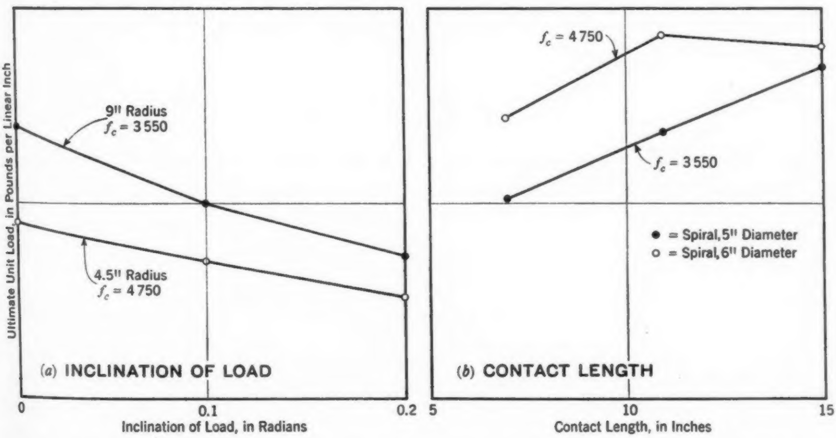

FIG. 7.—EFFECT OF INCLINATION AND CONTACT LENGTH ON UNIT LOAD; CONSIDÈRE ROLLERS

In these tests the upper limit of the straight-line part of the deformation curves occurred between 50% and 70% of the ultimate loads for concretes in excess of 2,000 lb per sq in. Failure was characterized by a vertical crack extending from one contact face to the other. The occurrence of this crack could be forecast by a rapid increase in the rate of deformation immediately

before its appearance. Maximum sets in compression noted upon two applications and release of about 30% of the ultimate load were as follows:

Ultimate strengths of plain concrete, f_c', in pounds per square inch	Set, in inches
990	0.013
1,290	0.008
3,550	0.004
4,750	0.002

It may be noted further that an excessive inclination of the load may be serious, since the ultimate loads show a marked reduction with increases in the inclination (Fig. 7(*a*)). The use of rollers containing more than one spiral unit in order to increase the contact length shows some improvement over the single unit rollers.

Fig. 7(*b*) contains the results of tests on 12-in. rollers (radius = 6 in.). Rollers 11 in. long required 2 spiral units, and those 15 in. long required 3 spiral units. These units were overlapped 1 in. when the spiral diameter was 5 in. and 2 in. when the spiral diameter was 6 in.

Design

Mesnager Hinges.—The design procedure for Mesnager hinges developed by Admiral Moreell (3) makes use of the following two equations for hinges with bare bars, presented by Messrs. Parsons and Stang (2):

$$f_s = \frac{T}{A}\left[\frac{K}{\cos\theta} \mp \frac{Q}{\sin\theta}\left(\frac{l}{r}\right)\right] \pm \frac{V}{A}\left[\frac{L}{\sin\theta} \pm \frac{R}{\cos\theta}\left(\frac{l}{r}\right)\right] + \frac{2\,E_s\,\phi}{\left(\frac{l}{r}\right)} \quad \ldots (1)$$

$$f_s' = \frac{T}{A\,(\cos\theta)} + \frac{V}{A\,(\sin\theta)} \quad \ldots (2)$$

in which: f_s and f_s' are the maximum, and the direct, compressive steel stress in a Mesnager hinge; A = the total bar area at the opening of the hinge; and

$$K = \frac{(l/r)^2}{(l/r)^2 + 12\tan^2\theta} \quad \ldots (3a)$$

$$Q = \frac{12\tan^2\theta}{\tan^2\theta + (l/r)^2} \quad \ldots (3b)$$

$$L = \frac{\tan^2\theta\,(l/r)^2}{(l/r)^2\tan^2\theta + 12} \quad \ldots (3c)$$

and

$$R = \frac{12}{12 + \tan^2\theta\,(l/r)^2} \quad \ldots (3d)$$

Eq. 1 (based upon fixed end conditions) yields maximum unit stress values as determined by adding the direct stresses and the superimposed bending stresses caused by T, V, and ϕ (the rotation of the hinge), whereas Eq. 2 provides a direct stress computed on the assumption of hinged ends at the face of the concrete for the bare bars, and neglecting ϕ. It has been recommended

(3) that the maximum stress computed by Eq. 1 be kept within the tensile yield point and that the direct stress computed by Eq. 2 should not exceed 30% of the tensile yield point (12,000 lb per sq in. for intermediate grade steel). The values computed by Eq. 1 agree well, and in general are slightly on the side of safety, with the results of tests shown in Fig. 2(*b*). The writer proposes that, for simplicity, design should be confined to the use of Eq. 1 by merely limiting the stress from Eq. 1, without rotational stress $\left(\frac{2 E \phi r}{l}\right)$ to 30% of the tensile yield point instead of using Eq. 2. If this is done, the design chart shown in Fig. 8 can be used for all usual conditions in which the bars are contained in two planes.

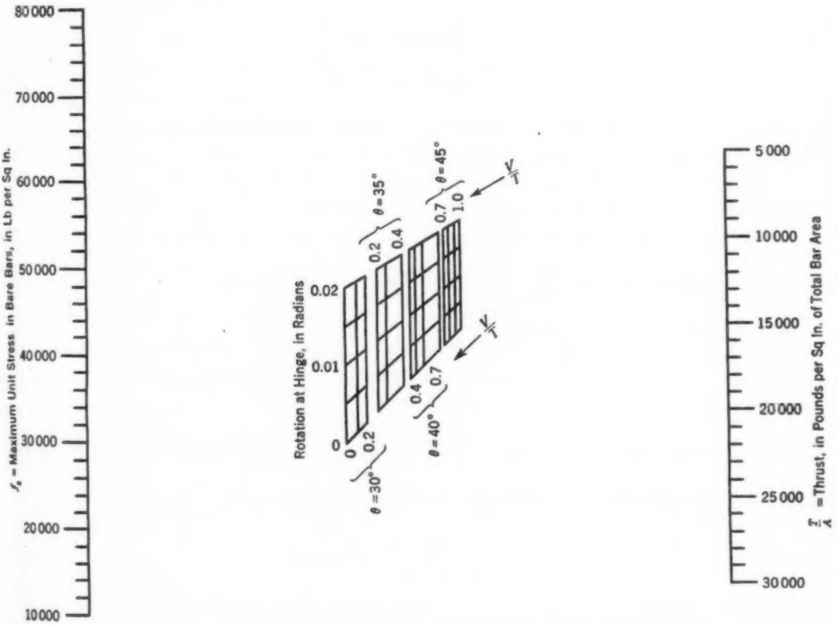

FIG. 8.—DESIGN CHART FOR MESNAGER HINGES; $\frac{l}{r} = 40$

The chart has been developed to provide values of θ equal to 30°, 35°, 40°, or 45° which will yield the lowest stresses consistent with Eq. 1. Hence, if it is best to use 35° instead of 30°, the chart will show intersections on a straight edge with the given $\frac{V}{T}$-ratio and θ in the region where $\theta = 35°$ only.

After determining the proper values of θ and $\frac{T}{A}$ for $\phi = 0$ and $f_s = 30\%$ of the specified yield point, the maximum stress should be checked against the tensile yield point by using the same diagram but including ϕ. If f_s exceeds

the tensile yield point, $\frac{T}{A}$ must be redetermined by using the tensile yield point for f_s with the same values of $\frac{V}{T}$, ϕ, and θ.

If it is desired that an allowance be made for the concrete covering, the thrust taken by the concrete should be computed from $0.32 f_c' \left(1 - \frac{V}{T}\right)$ times the minimum concrete section and this value deducted from the total thrust prior to computing the steel area from the $\frac{T}{A}$-ratio of the chart.

The percentage of transverse reinforcement may be computed by the following approximate equation given by Messrs. Parsons and Stang,

$$f_v = \frac{\frac{T}{2}\tan\theta + \frac{V\,a}{j\,d}}{0.005\,a\,b + A_v} \quad \text{(4)}$$

in which: f_v = unit stress in transverse steel in pounds per square inch; T and V = total thrust and shear; $a\,b$ = effective area of web in resisting tensile forces (a being taken along axis of hinge); $j\,d$ = lever arm of internal resisting couple; and A_v is the transverse steel area contained in $a\,b$. With $a = 8\,D''$ in which D'' = diameter of longitudinal hinge bars (2); $j = 0.9$, and p_v, the transverse steel ratio, $= \frac{A_v}{a\,b}$, the formula for p_v becomes,

$$p_v = \frac{T\left[\frac{\tan\theta}{2} + (8.9)\left(\frac{D''}{d}\right)\left(\frac{V}{T}\right)\right]}{8\,D''\,b\,f_v} - 0.005 \quad \text{(5)}$$

For covered bars, the term $\left[(8.9)\left(\frac{D''}{d}\right)\left(\frac{V}{T}\right)\right]$ does not need to be included provided the bars are adequately anchored; and

$$p_v = \frac{T\,(\tan\theta)}{16\,D''\,b\,f_v} - 0.005 \quad \text{(6)}$$

from which p_v may be determined for hinges with covered bars.

Considère Hinges.—Considère hinges may be designed by obtaining the percentage of longitudinal and spiral steel from current spiral column formulas and then by limiting the deformation in the extreme fibers at maximum rotation to a permissible proportion of the ultimate value. The following design formulas for symmetrically reinforced hinges have been developed previously (11) by the writer on the assumption of a linear distribution of strain, a parabolic (2d degree) distribution of concrete stress, and a limit of 50% of the ultimate compressive unit deformation in the concrete at the rotation ϕ:

$$\frac{P}{A_g} = \frac{5}{12} f_c'\,k\left(\frac{d}{d'}\right) + \frac{E_s\,p_g\,\epsilon_c'}{4\,k}\left(2\,k - \frac{d'}{d}\right) \quad \text{(7)}$$

$$\frac{\phi}{h} = \frac{\epsilon_c'}{2\,k\,d} \quad \text{(8)}$$

and

$$f_s = \left(\frac{1-k}{k}\right)\frac{\epsilon_c' E_s}{2} \quad \text{(9)}$$

in which A_g = the gross area of a spiral column or a Considère hinge; d = the effective depth, and d' the full depth, of the hinge section; E_s = Young's modulus for steel; ϵ_c' = ultimate compressive unit deformation of concrete; and k = ratio of distance from the neutral axis to the outer fiber. Occasionally, the tensile stress, occurring in the steel during rotation, is of prime importance and must be checked in order to obtain the proper grade of longitudinal steel. A design chart for the use of Eqs. 7, 8, and 9 has already been developed (11) for a deformation limit of 50% of an assumed ultimate of 0.004 in. per in. in the concrete.

The quantity of spiral steel should be large enough to act as diagonal tension reinforcement in a member under combined shear and direct stress. With the section primarily under compression, the maximum shearing unit stress may be taken at 1.5 times the average, without appreciable error. Thus, the formula for use of the spiral as web reinforcement may be taken as,

$$\frac{2\,s\,b}{(D')^2} = \frac{N\,\pi\,f_v}{\left(\frac{T}{2\,A_g}\right)\left[\sqrt{1+9\left(\frac{V}{T}\right)^2}-1\right]-C'\,f_c'} \quad \text{(10)}$$

in which s = pitch of spiral; b = breadth of hinge; D' = diameter of rod forming the spiral; A_g = gross area of hinge; f_v = stress in spiral; $C'\,f_c'$ = allowance for concrete in diagonal tension; T and V = total thrust and shear, respectively; and N = number of spiral units in A_g. With the steel stress limited to 16,000 lb per sq in. this becomes

$$\frac{s\,b}{(D')^2} = \frac{50{,}300\,N}{\left(\frac{T}{A_g}\right)\left[\sqrt{1+9\left(\frac{V}{T}\right)^2}-1\right]-2\,C'\,f_c'} \quad \text{(11)}$$

from which the spacing and diameter of the rod may be checked against the values obtained from the design as a spiral column under direct thrust.

Considère Rollers.—Mr. Scott (12) recommends that the mid-section of the Considère roller (Fig. 6) be designed with $\frac{2}{3} f_c'$ as the limit for the maximum compressive stress on the cross-sectional area. The appropriate radius will be determined from the usual relationship between load and radius for rollers acting on a plane ($P = C\,r$, in which P = load in pounds per linear inch, r = radius of roller, and C = a constant given by Mr. Scott as 342 for a concrete mixture at least as rich as 1 : 1.5 : 3). The line $P = 342\,r$ is plotted in Fig. 9, which provides a factor of safety of about 4 on the ultimate of the tests shown and greater than 2 as compared to the loads at initial cracking.

Messrs. Taylor, Thompson, and Smulski suggest that the percentage of spiral steel (or hooping) should be sufficient to resist a total tension equal to one third of the total concentrated reaction acting on the roller, with the use of

lead sheets, of appropriate width, between the contact faces as a substitute for the curvature.

It has been shown (14) that two equal and opposite concentrated forces acting along a diameter of a roller develop a uniform tensile stress of $\frac{P}{\pi\,r}$ pounds per square inch on a plane containing the contact lengths. Therefore, the total tensile force normal to the diametral cross section containing the contact lengths would be $\frac{2\,P}{\pi}$. This is the criterion for initial tensile cracking beyond which a truly circular roller can no longer be considered as acting as a unit within the elastic range of stress.

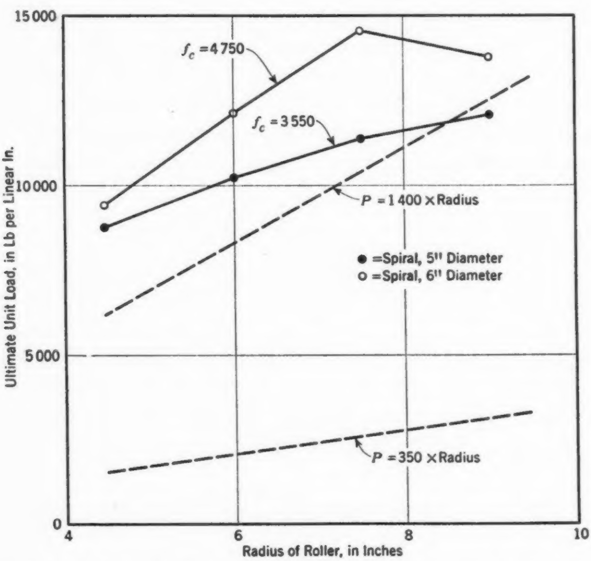

FIG. 9.—ROLLER RADIUS VERSUS UNIT LOAD ON THE CONTACT LENGTH OF CONSIDÈRE ROLLERS

Practical design for such rollers requires that the roller be narrow as shown in Fig. 6. This changes the uniform tension on the vertical section to a distribution having a unit stress higher than $\frac{P}{\pi\,r}$ near the top and bottom. When the breadth is $\frac{4\,r}{\pi}$, the distribution of tensile stress on the vertical section will be the same as that of the compressive stress on the central horizontal cross section and also not seriously different from a uniform distribution. A further reduction in the breadth would be questionable since the concrete (prior to cracking) or the steel (at the ultimate load) soon would become excessively over-stressed near the top and bottom. This may be noted in Fig. 9, since the breadth varies from 1.56 r for the 4.5-in. radius to 0.78 r for the 9-in. radius. Aid in resisting this action, after tensile cracking, is supplied by the grid stirrups placed at top and bottom of the spiral.

In general, it appears best to supply sufficient spiral steel to resist two thirds of the concentrated reaction coming to one contact length, with the limitation of $\frac{4r}{3}$ for the minimum breadth. The addition of grid stirrups at top and bottom of the spiral, each to resist a tensile force of one third of the concentrated reaction, would be a desirable safeguard against the possible non-uniformity of tensile stress distribution. It is apparent from Fig. 7(*a*) that it would be well to limit the maximum inclination of the resultant to not more than 0.05 radian with the center line of the spiral, and from this inclination determine the diameter (or height) of the roller with the aid of the known horizontal movement.

Acknowledgment

The experimental material illustrated in this paper is an abridgment of data resulting from an investigation of reinforced concrete hinges conducted by the writer for the Engineering Experiment Station, University of Maryland.

APPENDIX I

Notation

The following notation conforms essentially with American Standard Symbols for Mechanics, Structural Engineering, and Testing Materials compiled by a Committee of the American Standards Association with Society representation, and approved by the Association in 1932:

A = area; total bar area at the opening of a Mesnager hinge, in square inches:

- A_g = gross area of spiral column or Considère hinge;
- A_v = area of transverse reinforcement in the web area $a\,b$ of a Mesnager hinge;

a = effective length for tensile resistance along the common axis of members adjoining the hinge;

b = breadth of the hinge section;

C = coefficient; experimental coefficient applied to the radius of Considère rollers:

- C' = coefficient of the ultimate strength f_c' for diagonal tension resistance of concrete;

D = diameter of spiral, in inches:

- D' = diameter of the rod forming the spiral;
- D'' = diameter of the longitudinal bars;

d = depths; distance from the outer fiber to the point of application of the resultant tensile stress of a reinforced concrete member:

- d' = full depth of hinge section;

E = elasticity modulus, in pounds per square inch:

- E_s = modulus of elasticity for steel;

f = unit fiber stress, in pounds per square inch:

f_c' = ultimate strength of plain concrete;

f_s = maximum compressive steel stress in a Mesnager hinge, or the tensile stress in a Considère hinge;

f_s' = direct compressive stress in Mesnager hinge bars computed on an assumption of pin-connected ends;

f_v = stress in the transverse reinforcement of Mesnager hinges or in the spiral of a Considère hinge;

h = height of hinge opening, in inches;

j = ratio of the lever arm of the resisting couple in a reinforced concrete beam to the distance between the outer compressive fiber and the point of application of the resultant tensile stress;

K = a substitution factor in Eq. 1 (see Eq. 2*a*);

k = ratio of the distance from the neutral axis to the outer fiber of a reinforced concrete beam to the distance from the outer fiber to the point of application of the resultant tensile stress;

L = a substitution factor in Eq. 1 (see Eq. 2*c*);

l = length of a bar not completely embedded in concrete at the hinge opening, in inches $\left(\frac{l}{r} = \text{slenderness ratio}\right)$;

N = number of spiral units within an area, A_g;

P = total load on a hinge or roller, in pounds;

p = steel ratio:

p_v = transverse steel ratio in Mesnager hinges;

p_g = longitudinal steel ratio in spiral columns, based upon gross area;

Q = a substitution factor in Eq. 1 (see Eq. 2*b*);

R = a substitution factor in Eq. 1 (see Eq. 2*d*);

r = radius of gyration of a bar at the hinge opening, in inches $\left(\text{slenderness ratio} = \frac{l}{r}\right)$;

s = pitch of a spiral, in inches;

T = total thrust component of the load P, in pounds;

V = total shear component of the load P, in pounds;

ϵ = unit deformation, in inches per inch:

ϵ_c' = ultimate compressive unit deformation of concrete;

θ = angle between a sloping bar and the center line of a Mesnager hinge, in degrees;

ϕ = the rotation of a hinge, in radians.

APPENDIX II

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PAPERS

PERMISSIBLE COMPOSITION AND CONCENTRATION OF IRRIGATION WATER

By W. P. Kelley,[1] Esq.

Synopsis

The limit of permissible salt content of irrigation water is greatly influenced by variables inherent in the soil, the climatic conditions, and the kind of crops grown. It is of the greatest importance to apply saline irrigation water in quantities in excess of the crop requirements, in order that some leaching of the root zone will take place. Therefore, the maintenance of good drainage condition in the soil is very important. Salts, whether native to the soil or applied in the irrigation water, cannot be removed effectively unless water can percolate through the soil, and this can never be accomplished adequately where the ground water is near the surface.

From the standpoint of irrigation supplies, there are two considerations of outstanding importance to irrigated agriculture, namely—(1) the physical availability of the irrigation supply, its cost of delivery, and abundance; and (2) the so-called quality of the water. The word "quality" as applied to irrigation water is not well chosen because it may refer either to the "ratio of dissolved constituents" or to "total concentration." The ratio of dissolved components does not have a fixed value in determining suitability for irrigation, because the influence of this ratio is affected considerably by the total concentration. Neither is the total content of salines highly determinative, because, within certain ranges of concentration, the kind of salines present has fully as much (perhaps more) influence than total concentration. Therefore, an irrigation water may be unsuitable because the kind of its components or its total concentration is unfavorable, or for both reasons. Hence the expression "composition and concentration" is preferable to the word "quality" as applied to irrigation waters.

Note.—Written comments are invited for immediate publication; to insure publication the last discussion should be submitted by **August 15, 1940.**

[1] Prof. of Soil Chemistry, Univ. of California, Berkeley, Calif.

Early in history it must have been realized that the salinity of irrigation water is a matter of some importance. Otherwise, it would be difficult to account for the fact that irrigation supplies have frequently been developed at relatively great expense when other supplies were near at hand—as, for example, along the ocean front or near saline lakes. However, recognition of the fact that comparatively low concentrations of salines may also be important is more recent. In fact, as experience has accumulated and scientific investigations have progressed, the accepted limits of salinity have tended to diminish progressively. In many instances, water which at one time was thought to be entirely safe for irrigation has either been abandoned or is under suspicion.

The late E. W. Hilgard,[2] M. Am. Soc. C. E., did the pioneer work in formulating knowledge on this subject, and laid the foundation for most of what is known about it today. He realized at the very beginning of his important investigations on irrigated agriculture that the farmer must reckon with the dissolved salines of irrigation water. Soon after taking up his work in California, in 1875, Hilgard began the analysis of irrigation waters, and from that date to the present time irrigation supplies have been freely analyzed for interested farmers in California. It is well to recall the pioneer work of Hilgard and to bear in mind that practically everything that has been done on this subject since his time has been chiefly in the nature of refinements of the fundamental ideas which he developed and advocated through a long period of highly productive research. Not always were Hilgard's ideas entirely palatable to farmers or promoters, but rarely has time failed to show their essential soundness.

Although Hilgard was a true pioneer in this field, others soon caught his vision. R. H. Forbes[3] and his successors in Arizona, and F. S. Harris[4] and his collaborators in Utah, have long recognized the importance of dissolved salines in irrigation water, and the same is true of certain members of the U. S. Department of Agriculture, notably T. H. Means[5] and C. S. Scofield.[6] Therefore the subject is far from new.

During quite recent years the subject of composition and concentration of irrigation water has come to the forefront. In fact, certain workers have concluded that a very high percentage of irrigated land in the United States (estimated at more than one half) is threatened with becoming excessively saline, owing to the salt content of the irrigation water. The writer does not subscribe entirely to this apprehension; the estimate is much too high, and unnecessary alarm has been aroused in certain sections.

It is perfectly true that large areas of irrigated soils have become excessively saline since the time irrigation was first introduced. For example, it is estimated that several hundred thousand acres of the more than six million acres

[2] Annual Repts., Agricultural Experiment Station, Univ. of California, 1878–1902.
[3] *Bulletin No. 44*, Agricultural Experiment Station, Univ. of Arizona, 1902.
[4] *Bulletin No. 139*, Agricultural Experiment Station, Univ. of Utah, 1919.
[5] *Circular No. 10*, U. S. Dept. of Agriculture, 1903.
[6] *Bulletin No. 40*, Dept. of Public Works, State of California, 1933.

have been thus injured in the State of California, and areas of substantial size have been affected in practically every other irrigated state. However, the evidence is not convincing that the salts that have become concentrated in the soil have originated chiefly in the irrigation water that was applied. It is true that there are certain localities in which the injury has been clearly produced by the salines of the water applied, and other areas where severe injury from this cause is more than a reasonable prospect, either immediate or remote; but the greater part of the injury thus far has come about through the capillary rise of salts from the deep subsoil, made possible by a high water table. Imperfect drainage conditions threaten irrigated agriculture far more seriously than salinity of irrigation supply.

Nevertheless, salinity of irrigation water is a matter of some importance. Unfortunately, however, it is difficult to name the exact limits of permissible concentration of salines. The principal reason why this is not feasible at present is that too many variables are involved, the range of which is very great. At best the irrigation engineer can set up only tentative limits, and at present these must be based mostly on practical experience in the localities in question. This will probably always have to be the case, because permissibility can never be a fixed quantity applicable any and everywhere, but a relative matter depending on soil, crop, and climate. A reasonably safe water for one soil or crop may be very questionable for another.

To illustrate: A certain water, which has been used in a few orange orchards near Riverside, Calif., has produced severe defoliation and other injury to the citrus trees. However, it was found that alfalfa can be grown quite successfully with the same water. A second example is that of a 10-acre orange orchard near Lindsay, Calif. Some years ago, after having been irrigated successfully with non-saline water for a period of about twelve years, this orchard was irrigated with water from a well that contained a rather high concentration of chlorine. In one section the citrus trees became almost completely defoliated after this well water had been used for only two or three years, whereas in another part of the same grove the trees were scarcely affected at all. The only known difference between the two parts was in the soil. Where the injury was severe the soil (San Joaquin sandy loam) was shallow, being underlaid with an impervious hardpan about 2.5 ft from the surface. On the other hand, where the injury was only slight, the soil (Hanford fine sandy loam) was porous and deep, no hardpan being found. These two instances were spectacular, but in many other cases the effects have been much less pronounced and are often difficult to determine.

In a third locality, lemon trees were found to be injured, decidedly, by water containing somewhat less than 1 ppm of boron, whereas beans and alfalfa grown as intercrops showed no detectable injury. Differences in the response to boron of different kinds of crops are sometimes great indeed, as Eaton has clearly shown in controlled sand cultures at Riverside.

Many other instances could be cited, but these will suffice to indicate that the character of the soil to which the water is applied and the kind of crop that

is grown have very great influence on the question of permissible salinity of irrigation water. On a given soil, water that is entirely safe for one crop may produce serious injury for another, and on soils of wide dissimilarity severe injury may be produced on the one and practically none on the other.

The soil and crop are by no means the only important variables. Climate is probably just as important. The rainfall has a tremendous influence. For example, a lemon grove near La Habra, Calif., is irrigated with a rather saline water. During each of two springs following winters of heavy rainfall, the soil solution has been found to be no more concentrated than that of groves irrigated with good water. However, in the autumn, before the rainy season, there was considerable difference. Therefore, rain may leach out whatever salines accumulated during the previous irrigation season. In such cases the irrigation engineer must reckon chiefly with the concentration that accrues from a single year of irrigation; but where the rainfall is too light to effect much leaching, the soil solution, after a few years of irrigation, may become considerably more concentrated than it was at the close of the first year of irrigation.

However, where saline water is being applied under light rainfall, it should not be inferred, necessarily, that the concentration of the soil solution will continue to increase indefinitely. If the soil is reasonably porous the irrigation water itself may effect a substantial degree of leaching and thus transport, down below plant roots, some of the salines that have been introduced previously. Obviously, the permeability of the soil is a large factor in all such cases. With soil that is underlaid with an impermeable hardpan near the surface, manifestly little leaching can occur, and in such case practically all the salines applied must remain in the soil; and such soil will ultimately become highly charged with salts irrespective of the rainfall. If the subsoil has low permeability the tendency will be in the same direction, although perhaps less aggravated. Since soils vary enormously in their permeability, variations in the permissible limits of salinity must be considerable for this one reason alone.

Finally, the effects of salines appear to be influenced by another variable—the atmospheric conditions under which the crop is grown. If the rate of evaporation is rapid, obviously water must be applied more frequently than in relatively cool, moist climates. In such cases (other factors being equal) the total quantity of salines actually applied in a given period of time will be influenced by the climate. However, there is some indication that the effect on the crop itself is influenced by the atmospheric conditions; but this type of effect is in need of further investigation.

At any rate, one can state, safely, that variations in soil type, kind of crop grown, and rainfall are all potent in influencing the effects and acceptability of saline irrigation water; and since the complex differences in soil, crop, and climate present an intricate series of variations, it is scarcely probable that generally applicable limits of permissible salinity can ever be established. Each locality is largely a problem unto itself. Also the method of application has some influence. Leaching certainly will tend to be most uniform and effective if the water is applied by the flooding or sprinkling system; but, of

course, the quantity of water applied has most influence. If applied by the furrow method, the lateral movement of moisture, in some instances, causes great lateral inequalities in the distribution of soluble salts, especially in the top soil. In certain cases soil scientists have found a ten-fold greater concentration between irrigation furrows than directly below the furrows. This type of inequality of distribution sometimes extends to a depth of 3 ft or more.

That the salts applied in the irrigation water may penetrate to very great depths, and therefore not remain permanently in the root zone, has been shown clearly by the results of an investigation on an orange orchard near Riverside. This orchard had been irrigated with fairly saline water since 1910. The annual rainfall is comparatively light, being about 11 in. The irrigation water was applied by the furrow method and the quantity per irrigation was similar to that of the general commercial orchard practice of that locality—about 30 acre-in. per yr. Certainly the farmer did not intentionally apply excessive quantities of water. The soil and subsoil of this orchard are sandy loams to great depth, and free from hardpan. The ground-water level is at least 150 ft below the surface.

When this soil was sampled to a depth of approximately 40 ft, it was found that the concentration of salines that originated in the irrigation supply, although somewhat variable in the samples from different sample holes, nevertheless was remarkably constant throughout the entire depth. It is true that there were zigzags in the curve of distribution, but the trend was toward a constant composition throughout the depth sampled. It is interesting to note that, on the average, the concentration of the soil solution, computed for a moisture content corresponding to that of field capacity of the soil, was somewhat less than three times the concentration of the water applied. The net influence of evaporation and transpiration, on the one hand, which tended to increase the concentration of the soil solution, and the downward movement and dilution of salines by rains and by irrigation water, on the other hand, which tended to reduce the concentration, jointly produced an increase in the concentration of the soil solution of only approximately three-fold that of the water applied. The fact that the concentration of the salts tended to be constant throughout the depth of 40 ft strongly indicates that this particular irrigation water could be applied to this soil indefinitely without producing a soil-solution concentration much in excess of three times that of the water applied.

One of the critical points in this entire question is not so much the salinity of water that is applied as the resulting effect that will be produced on the soil solution. The effect on the soil solution produced by a given saline water is highly variable. The variation depends almost entirely upon the properties of the soil to which it is applied, the rainfall, the frequence and quantity applied, and the method of application. As indicated previously, the method of application may be of considerable importance, particularly if there is a plow-sole (a firm, rather impervious, layer created by too frequent plowing at the same depth), or if the soil permits only slow penetration of water. In these cases the lateral movement of water may result in the development of much high con-

centration of salts between irrigation furrows. In the aforementioned grove, although the soil is quite porous and practically free from plow-sole, nevertheless considerable difference was found in the twenty individual samples that were drawn from each of two different parts of this grove. In one part of the orchard the injury to the orange trees was much more severe than in other places, and here the concentration of soil solution was considerably higher, particularly in the upper part of the soil. In another grove irrigated with this same water, the orange trees were literally killed by the salts of this water, whereas in other parts of the same orchard the trees (although somewhat injured) are still bearing fruit after the continued use of this water for more than thirty years. Undoubtedly, slight differences in soil, although difficult to measure, have a profound influence on the movement of salines and therefore on the effects that will be produced on crops.

It seems scarcely necessary to reiterate that results such as the foregoing may not be obtained on all soils and under all conditions. However, this much can be stated definitely: If the water penetrates below the depth of the crop roots, it is certain that the salts will be displaced downward to some extent after each irrigation, and will finally make contact with the ground water; or a perched water table will develop. Furthermore, this will be the result even though the soil is relatively heavy, provided penetration can take place at all. If the movement of water through the soil is slow, a longer period of time after the water is applied will be required to bring about equilibrium of distribution. In the aforementioned orchard approximately constant moisture conditions were found from the surface of the soil down to a depth of 40 ft three days after irrigation. This is further evidence of the freedom of movement of moisture through this soil.

On the other hand, if the amount of water applied is no more than enough to wet the soil of the root zone, then the salts applied must necessarily accumulate in the root zone and ultimately will produce an injurious concentration even though the salt content of the water is low. Therefore, it is extremely important, where saline water is used, to apply more water than is necessary merely to wet the root zone. A certain degree of leaching must be provided by applying water in excess of the water requirements of the crop; otherwise the root zone will inevitably become highly charged with salts. Obviously, the more saline the water applied, the sooner will injurious concentration result in the soil solution, but in such cases water should not be applied sparingly. The more saline the water is, the more should be applied in order that leaching may occur every time water is applied.

Irrespective of where the water is applied, or the kind of crop grown, boron is one of the most toxic constituents found in the irrigation waters of California. It is doubtful whether water containing more than 0.5 ppm of boron can be applied to orchard soils continuously without ultimately producing some injury. The on-set of the injury will appear after varying periods of time, depending on the soil, frequence of irrigation, and to a lesser extent on the species of fruit trees grown, the fertilization, and general management of the orchard.

Among the more common constituents of irrigation water, chlorine is perhaps the most to be feared, but different kinds of crops vary widely in their response to chlorine. With citrus trees grown in California the water should usually not contain more than 150 to 200 ppm chlorine. Soluble carbonate is more toxic than chlorides, but it is much less widely present in irrigation waters. Sulfates are somewhat less toxic than chlorides.

In conclusion, a word is in order about sodium. There certainly is some importance to the ratio of sodium to calcium in irrigation water. This is true, because that ratio has an important bearing on the absorption of sodium by the soil. If the amount of absorbed sodium in the soil becomes substantial, the physical properties of the soil tend to become affected adversely; and if the proportion becomes extreme, the permeability of the soil will be impaired seriously. Present information indicates that very little sodium will be absorbed by the soil unless the ratio of sodium to calcium present in the soil solution is greater than 2 : 1. However, it is important to realize that the ratio of sodium to calcium in the actual soil solution may become substantially greater than it is in the water applied. This is due to the fact that calcium is absorbed by crops, and is therefore removed from the soil to a much greater extent than sodium; or it is precipitated as insoluble carbonate, which produces the same type of effect in this ratio.

Perhaps enough has been said to indicate the general trend of present-day knowledge and to give some idea of the great complexity of this subject. It is gratifying to find that the engineer is taking an interest in this highly complicated subject, in its chemical, physical, and physiological aspects. Every one is working toward the same goal, and each should profit by the experiences of those working in related lines.

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AMERICAN SOCIETY OF CIVIL ENGINEERS

Founded November 5, 1852

REPORTS

FLOOD-PROTECTION DATA

PROGRESS REPORT OF THE COMMITTEE

The Committee on Flood-Protection Data herewith submits its annual progress report for the year 1939. For convenience the findings will be listed under various headings corresponding with divisions of the Committee's work.

I. Hydro-Meteorological Studies

At its meeting in Washington, D. C., on September 9, 1939, the Committee devoted its attention chiefly to the new hydro-meteorological methods of determining maximum rates of flood runoff, which are aimed at ascertaining the maximum storm rainfall on the basis of what is physically possible of occurrence over any selected watershed and the runoff resulting therefrom. Progress has been made during the current year in this new field with respect to increasing the areas covered in various parts of the United States as well as to improving the technique. These studies, which were briefly described in the progress report of the Committee for 1938,[1] are primarily intended to supply the Corps of Engineers, United States Army, with maximum runoff figures needed in planning flood control, and are being carried out partly by the Corps' own forces and partly by Weather Bureau personnel working with War Department funds transferred to that Bureau.

The hydrological phase of the studies concerns:

(1) The gathering and analyzing of storm rainfall data for the storms selected for study; the determination of the beginning and ending of storm-rainfall periods by reference to the observer's original record and by local research, including personal interviews where necessary; the preparation of mass-rainfall curves; depth-duration curves of rainfall for successive increments of time in natural sequence; "maximum depth-duration" curves representing the maximum depth of rainfall that occurred in various periods of time irrespective of natural sequence in the storm; and isohyetal maps to determine the pattern and concentration of rainfall during periods producing the maximum depth-duration relationships. The Corps of Engineers has about 200 engineers assigned to this class of work, distributed in some 47 district-engineer offices. This number includes supervisory personnel in division offices and in the Office of the Chief of Engineers, U. S. Army.

[1] *Proceedings*, Am. Soc. C. E., January, 1939, pp. 95–96.

(2) Study of runoff conditions by this same personnel, using stream-flow data, the unit-graph method, and personal reconnaissance of topographic, infiltration, and vegetation conditions.

The meteorological phase of the studies, which is carried on by personnel of the Hydro-Meteorological Section of the Weather Bureau's River and Flood Division, includes:

(*a*) Topography—with respect to orographic effects; the lifting of warm, moist air to higher altitudes; and conditions promoting convective activity;

(*b*) Climatology—which requires subdivision of the basin into zones, as, for instance, into subhumid, semi-arid, and arid, all of which may be represented within one basin, each having rainfall and storm characteristics of its own;

(*c*) Storm-Types—these will be different in each zone as regards area and rainfall intensities;

(*d*) Selection of Storms—for detailed study with a view to ascertaining maximum rainfall intensities and distribution as actually recorded; the limitations imposed by topographic and climatologic conditions as affecting the transposition, shifting, and reorienting of such storms into more critical positions over the basin;

(*e*) Transposition of Storms—from adjacent basins, its applicability, and limitations;

(*f*) Snow Depth and Runoff—and their relative importance as compared with other flood-promoting factors;

(*g*) Precipitable Water Content—in a column of air 5 km in height for various temperature distributions in the column;

(*h*) Duration Limits of High Rates of Precipitation—which are contingent on rate of travel of warm, moist air masses toward cold air fronts, and on precipitable water content.

Great assistance in correlating the immense amount of rainfall data to be analyzed has been found in the "breakdown" of rainfall into 12-hr or even shorter periods, the use of time-area depth curves, the mass-diagram for plotting storm rainfall, and the enveloping depth-duration curve. The latter represents the maximum intensities for all durations as derived from the various storms studied in a basin, and is of assistance in determining as well as in delimiting the most critical sequence of rainfall that could occur.

II. Floods in Close Succession

The Committee again wishes to emphasize the importance of giving due consideration, in the design as well as in the operation of flood-control reservoirs, to floods occurring at intervals so short as to interfere with releasing appreciable volumes of stored water between floods. The phenomenon of two major floods in close succession, sometimes called "twin floods," is peculiar to many watersheds in the United States, notably to those bordering the Atlantic, Pacific, and Gulf coasts, as well as to inland watersheds lying in the paths of tropical storms or subject to heavy rainfall induced by tropical storms in adjacent areas. An 8-day or even shorter interval between two major flood peaks, such as has repeatedly been recorded, usually imposes a severer test on

the adequacy of a flood-control plan using reservoirs than does a single maximum flood. Rocky Mountain streams are exempt from this type of storm recurrences, but, on the other hand, are subject to cloudburst floods at even shorter intervals.

The operation problem in all such cases relates not so much to the impracticability of emptying reservoirs in the short interim between floods, as to releasing sufficient water from storage to enable some reduction to be effected in the peak rate of the second-flood crest. When, as often happens, streams below the reservoirs continue to run bank-full during most or all of the interim period, and release therefore is felt to be inexpedient, the operator will find himself faced at the approach of the second storm with the dilemma of having to release water too late to provide adequate storage capacity for handling the second-crest water effectively. Spillway discharge then becomes so large as to constitute a flood menace in itself and flood control has been defeated. A few examples of so-called twin floods are here cited:

(a) *Colorado River at Austin, Tex., September, 1936.*—A storm in the upper watershed caused a flood crest to reach Austin on September 22, causing a stage of 25 ft. A second storm lower down in the watershed, near Austin, produced a second flood crest of 32 ft on September 28. Both floods were destructive. Previously, crest stages of 25 ft and greater had occurred four times in 40 years.

(b) *Roanoke River at Weldon, N. C., August, 1928.*—Flood crests occurred on August 16 and 21, within 5 days of each other, with crests of 44.8 and 44.5 ft on the Weldon gage. These floods resulted from two tropical disturbances passing northward from Florida along the Atlantic Coast, and caused great damage. The magnitude of these floods is on the order of 6-yr frequency of occurrence.

(c) *Savannah River at Augusta, Ga., 1929.*—The floods of September 27 and October 2, with crest stages of 46.3 and 45.1 ft, respectively, were both caused by tropical disturbances. The rain giving rise to the first flood amounted to from 6 to 15 in. in the 34 hr ending 8 a.m., September 27, and that causing the second ranged from 5 to 10 in. in the 30 hr ending 6 p.m., October 2. The peak discharge on September 27 was estimated at 342,000 cu ft per sec, and that on October 2 at 350,000 cu ft per sec. Flood damage at Augusta begins at about a stage of 32 ft and becomes serious at 35 ft. The twin floods merged into one in their course down the coastal plain. Earlier the same year twin floods occurred at Savannah, Ga., on March 1, stage 37.2 ft, and March 6, stage 38.7 ft.

(d) *Southern California Floods, February–March, 1938.*—The great damage inflicted in Los Angeles County, despite the presence of thirteen flood-control reservoirs, was attributable to two intense storms in quick succession—the first on February 27 and 28, and the second on March 2 and 3. The latter provided the most intense rainfall and peak runoff rates. At the beginning of the maximum 24-hr period of runoff on March 2, all of the reservoirs were more than half full, their available storage capacities ranging from 13.7 to 30.3%. As the flood runoff on that date alone was in excess of the total reservoir capacity at ten of the thirteen reservoirs, the reduction in peak flow effected by the system

as a whole was necessarily small. In this instance, the period between storms was less than one day, a time entirely too short to permit the release of much stored water. This situation became aggravated further by the clogging of the outlets at six of the dams by debris brought down by the flood waters.

(e) *New York and Pennsylvania Floods of March, 1936.*—The storm of March 17–18 followed so closely upon the heels of the storm of March 11–12 that the resulting runoff from the later storm found streams already at high stages, thus causing high rates of runoff. Rivers affected by the twin floods were: The Delaware, the Lehigh, the Schuylkill, and the North and West Branches of the Susquehanna River. The Allegheny River on this occasion had three flood crests in succession—namely, on the 12th, 18th, and 28th—but none was very high.

III. Statistical Methods

In its annual progress report for 1938[2] the Committee expressed caution against placing undue confidence in statistical methods of determining the probable frequency of recurrence of floods of various magnitudes. This appears to have been misconstrued in some quarters to mean a wholesale condemnation of such methods. The Committee has consistently recognized that statistical methods have their proper uses, and, with a view to promoting their correct usage, was responsible in 1934 and 1935[3] for the publication of the more commonly used methods together with a discussion of their relative merits.[4] It senses, however, that for certain purposes these methods possess such decided limitations that their use is not to be recommended. One of these is to ascertain by extrapolation on probability paper the magnitude of great floods of very rare occurrence, such as for instance might occur in a period of 1,000 years or even longer, when the frequency curve is built up on statistical information covering only 20 or 30 years. Errors from this source have been numerous in the past, and, in some cases, serious. The magnitude of the error so involved may be judged from the data presented in the paper by William P. Creager,[5] M. Am. Soc. C. E.

Another use of the flood-frequency method which the Committee deprecates is for computing what is known as the "average annual flood damage," in which specific damage figures are assigned to floods of selected magnitudes. The dangers of this method become obvious when it is realized that probably no aspect of the flood-control problem has shown greater susceptibility to variation with time than the damage inflicted by a flood of a selected magnitude. Flood damage, by its very nature, is transitory in character because there is constant evolution in Man's uses of rivers and their flood plains. To assume, for convenience of computation, that the damage remains practically the same for each future recurrence of a certain flood magnitude is prima facie a fallacy because the damage inflicted by any flood never is a true "sample," mathe-

[2] *Proceedings*, Am. Soc. C. E., January, 1939, pp. 93–100.

[3] *Loc. cit.*, March, 1935, pp. 336–337; and February, 1936, p. 203.

[4] "Floods in the United States—Magnitudes and Frequencies," *Water Supply Paper No. 771*, U. S. Geological Survey.

[5] "Possible and Probable Future Floods," by William P. Creager, *Civil Engineering*, November, 1939, pp. 668–670.

matically speaking. To assume that the damage will become progressively greater with time on account of increasing population and more extensive flood hazards, as indicated by the past experience of certain cities, will always need careful checking. Many municipalities have adopted zoning regulations and converted flood zones into parks, and have taken other steps calculated to reduce rather than to increase future flood damage. Modern highway and railroad bridges, because of improved design and construction, are less subject to destruction by flood waters than were those built in the past; and future structures of this kind will be even more substantial if for no other reason than because they must meet steadily increasing traffic requirements. In many cases, a fixed stage-damage relation becomes a gratuitous assumption which, if made the foundation for arriving at the economic worth of a flood-control project, is apt to lead to erroneous conclusions.

The application of frequency methods to a graduated scale of crop damage for different-sized floods requires especial caution, as seasonal conditions and different harvesting practices introduce many variables. In flat river bottoms, a small depth of overflow may inflict as much crop damage as does a greater flood with twice or three times the depth of water. In narrow valleys, the higher stage, also, may not appreciably increase the area of over-flow. Summer and fall floods, for obvious reasons, cause more damage than winter or spring floods of equal magnitude; and a second flood on the heels of a crop-destroying flood—a common occurrence in Atlantic Coast streams—does relatively little damage compared with the first, although both floods may be of practically the same magnitude.

In a period of four years characterized by excessive solar radiation, such as has recently been witnessed, the expectation, meteorologically speaking, is for a succession of large floods. If such a period should happen to produce two floods of magnitude such as would statistically be expected to occur, say, only once in 500 years, this mathematical probability expectation becomes of purely academic interest. Floods, actually experienced and of considerable magnitude, constitute a sounder basis for a "project flood" on which to predicate expenditures to be borne by the taxpayer than hypothetical flood events derived from a frequency curve. A factor of safety may properly be added if engineering judgment indicates this to be a wise procedure.

IV. Inventory of Flood Data

Glenn L. Parker, M. Am. Soc. C. E., chief hydraulic engineer, U. S. Geological Survey, reports that substantial additions have been made to the Inventory of Flood Data mentioned in the progress report of the Committee,[6] the inventory having been started during 1938 with funds made available by the Public Works Administration. The field work for the special studies of floods and droughts was completed during 1939 and the data so secured are being reviewed preliminary to placing them in form for publication. In the Committee's opinion, the practical value of the data gathered is such that nothing should be permitted to interfere with the continuance of this work in

[6] *Proceedings*, Am. Soc. C. E., January, 1939, p. 95.

order that all principal watersheds in the United States may eventually be covered. The Committee feels that the Inventory of Flood Data started in 1938 by the Geological Survey is destined to become a valuable permanent source of information for public officials and engineers alike. Mr. Parker advises that a number of special reports on destructive floods are in process of compilation by his staff.

V. Recent Publications

Many reports and articles on floods, flood data, and methods of analysis have come to the attention of the Committee during the year. Taken collectively, the information so made available is exceptionally large and valuable. More than that, flood literature for 1939 brings out clearly the great strides that have been and are being made in the collection, development, and interpretation of flood data. The reports and papers reviewed are entirely too numerous to permit of enumeration, but those containing matter of special interest are here listed:

(*a*) *American Red Cross Reports on Relief Operations.*—During the year, seven reports on flood relief have been issued by the Red Cross which should prove valuable accessions to the library of any engineer concerned with flood-control economics. They contain many important facts, give in detail the cost of flood relief and rehabilitation, and shed an important light on that intangible item—the price of human misery incident to floods. The reports are entitled: "Spring Floods and Tornadoes of 1936," "Alabama-Mississippi Tornadoes and Floods of 1938," "West Texas Flood (Colorado River) of July 1938," "California Floods of 1938," "Disasters of 1938–1939 (Includes Ohio Valley Floods of February 1939)," "Eastern Kentucky Floods of 1939," "New York-New England Hurricane and Floods of 1938."

(*b*) *Flow of the Rio Grande and Tributary Contributions, 1938.*—This report, *Water Bulletin No. 8* of the International Boundary Commission, United States and Mexico, contains important contributions to flood-peak records on the Rio Grande not previously published by the Commission, and flood-frequency curves; also rainfall records on the Mexican and United States sides of the Rio Grande Watershed not previously published.

(*c*) *The Chattanooga Flood-Control Problem.*—This publication, *House Document No. 91*, 76th Congress, 1st Session, contains the report of the Board of Directors of the Tennessee Valley Authority, dated December 23, 1938, submitted to the President of the United States. Appendix B contains a description of past floods with quotations from early documents and newspapers. The graph on page 85 shows a remarkable decrease in the magnitudes of major floods in the course of 70 years, which, whether it reflects the marked changes in runoff conditions that have been brought about in the upper watershed or is purely fortuitous, is of extraordinary interest.

(*d*) *Low Dams.*—This manual of design for small water-storage projects, published by the Water Resources Committee of the National Resources Committee, September, 1938, contains, among other things, a revised map showing maximum flood runoff rates for different parts of the United States in per-

centages of the Myers scale, prepared by C. S. Jarvis, M. Am. Soc. C. E. In Appendix A a rational method of estimating flood flows is given by Merrill Bernard, M. Am. Soc. C. E., Chief of the River and Flood Division of the Weather Bureau. In Appendix I are listed 1,011 maximum observed flood discharges for streams in all parts of the United States.

(*e*) *Floods in the Canadian and Pecos River Basins of New Mexico, May and June, 1937.*—This is U. S. Geological Survey *Water Supply Paper No. 842*, 1939. It contains peak discharge determinations made at about 14 localities in addition to flood records obtained at 23 regular gaging stations. A summary gives records of all past floods in New Mexico streams for which authentic data are available.

(*f*) *Major Texas Floods of 1935.*—This report, U. S. Geological Survey *Water Supply Paper No. 796–G*, 1939, was prepared in cooperation with the Texas Board of Water Engineers. Extraordinary floods which occurred in May, 1935, on Seco Creek, caused by 22 to 24 in. of rainfall in 3½ hr, are described. Other floods described are those of June in the Colorado and Nueces rivers, and the Buffalo Bayou flood of December which devastated Houston, Tex.

(*g*) *History and First Annual Report of the Metropolitan Water District of Southern California*, by F. E. Weymouth, Hon. M. Am. Soc. C. E., Chief Engineer, 1939.— This report includes information on early floods, droughts, and rainfall cycles carried back to 1770.

(*h*) *Symposium on Floods* presented before the Commission of Potamology of the International Association of Scientific Hydrology at the Seventh Assembly of the International Union of Geodesy and Geophysics, was published in the *Transactions* of the American Geophysical Union, 1939, Part II. It consists of papers on recent advances in applied hydrology with reference to flood forecasting and the estimating of maximum flood flows possible of occurrence. Among the authors are the following members of the Society: Merrill Bernard, George D. Clyde, Gail A. Hathaway, W. G. Hoyt, C. S. Jarvis, Walter B. Langbein, Gerard H. Matthes, Carl G. Paulsen, Thorndike Saville, and Leroy K. Sherman.

(*i*) *The Maximum Probable Flood and Its Relation to Spillway Capacity*, by S. M. Bailey and G. R. Schneider, Assoc. Members, Am. Soc. C. E., in *Civil Engineering* for January, 1939.

(*j*) *Flood-Discharge Records Relating to Pennsylvania Streams*, by the Pennsylvania Department of Forests and Waters in cooperation with the U. S. Geological Survey, 1938.—This report is in the nature of a summary of all known authentic records of stage and flood discharge in the streams of the state, compiled by drainage basins in tabular as well as in diagrammatic form. In several instances, figures are given for great floods that occurred half a century or more ago. A study of maximum rates of runoff per square mile from different-sized drainage areas is appended.

(*k*) *California Highways and Public Works* for October, 1939, published by California State Department of Public Works.—This publication contains an illustrated article on the cloudburst floods of September, 1939, in the Coachella

and Imperial valleys, in California, which damaged state highways to the extent of $500,000.

(*l*) *The Flood Problem in Fire Prevention and Protection.*—This report, prepared and published by National Board of Fire Underwriters, New York, 1939, describes fire hazards in connection with floods.

(*m*) *High-Water Data—Floods of March, 1936, and September, 1938, in Massachusetts.*—This summary by the Massachusetts Geodetic Survey gives flood-height observations made after the hurricane storm of September 21, 1938, and comparisons with similar observations obtained after the flood of March, 1936, previously published. The work was done with funds made available by the Works Progress Administration.

(*n*) *Transient Flood Peaks*, by Henry B. Lynch, M. Am. Soc. C. E., in *Proceedings*, Am. Soc. C. E., for November, 1939.—This is a welcome contribution to the meager literature on cloudburst flood runoff. It describes the surge phenomenon produced by an abrupt increase of rainfall over a small area, popularly termed "wall of water."

(*o*) *Report of Cooperative Hydrologic Investigations*, by the Pennsylvania Department of Forests and Waters in cooperation with U. S. Weather Bureau, U. S. Geological Survey, and Pennsylvania Water Power and Resources Board, August, 1938.—This report sets forth the results of rainfall and runoff studies over a 2-yr period for the ultimate purpose of inaugurating a modern scientific flood-forecasting service for all communities along Pennsylvania streams. The appendix contains 60 graphs showing the results of the studies.

(*p*) *Report on Flood-Damage Data*, prepared for the Water Resources Committee of the National Resources Committee by a subcommittee.—This report is in the nature of a recommended check list of types of flood losses, indicating methods for evaluating such losses. It was issued in mimeograph form, March 15, 1939.

(*q*) *Possible and Probable Future Floods*, by William P. Creager, M. Am. Soc. C. E., *Civil Engineering*, November, 1939.—This is a recapitulation of the manner in which engineering knowledge of maximum rates of runoff from watersheds of various sizes has increased from 1890 to the present time. Commencing with the enveloping curve of record-breaking floods prepared in 1890, there are shown similar curves prepared in 1913, 1921, 1934, and 1939, followed by an attempt to forecast possible future increases in runoff rates. None of the diagrams is applicable to any given watershed. The evidence presented is impressive as regards the enormous increase in flood magnitudes that has been experienced with time and with increased facilities for recording, especially from the smaller drainage areas.

(*r*) *Maximum Possible Precipitation over the Red River Basin above Denison, Texas*, by the Hydro-Meteorological Section, River and Flood Division, U. S. Weather Bureau, in cooperation with the Corps of Engineers, U. S. Army, October 25, 1939.—This mimeographed article is one of a series of studies undertaken to supply information needed in estimating maximum flood runoff for use in planning flood-control works. The report is of particular interest because the watershed of Red River contains areas differing radically in storm-rainfall characteristics. These place definite limitations on the transposition of storms

over the basin. The general methods used in this class of studies are described under the heading, "Hydro-Meteorological Studies."

(s) *Floods of Ohio and Mississippi Rivers, January–February, 1937,* U. S. Geological Survey *Water Supply Paper No. 838, 1938.*—This is a 746-page volume containing a wealth of data admirably presented and of interest also because it embodies features that are novel in reports of this kind. It contains precipitation data and a discussion of the meteorological aspects; daily stages and discharges for the high-water period at about 250 stream-measurement stations; elevations above sea level of flood-crest stages along some 5,000 miles of stream channels; and a summary of peak discharges with comparative data for about 470 points of measurement. In addition, the report gives the volumes of water that were stored in the principal reservoirs in the watershed, and an analysis of the volumes of natural floodwater storage in river channels and on the flood plains. Of special interest to engineers requiring accurate data for the construction of flood-stage hydrographs are the tabulations showing stage and discharge at various hours for each calendar day for streams whose rate of rise and fall is so rapid that the usual once-daily figures do not adequately define the hydrographs. An appendix on "Flood Deposits" tends to show that the flood of January–February, 1937, exceeded in height any previous flood as disclosed by a geologic reconnaissance of the evidences left by the great floods of the past. A valuable feature is the presentation of numerous discharge hydrographs for the rivers in the Ohio Basin.

(t) *Biennial Report for 1937–38* of the State Engineer of Colorado, M. C. Hinderlider, M. Am. Soc. C. E.—This contains detailed information concerning the runoff during the floods in Bear Creek and Mt. Vernon Creek in 1933, 1934, and 1938; also similar data for the floods in the South Platte River and Big Thompson River, September, 1938. Comparative data for previous floods in these streams are presented.

VI. Cooperation Between Agencies

Worthy of special comment is the growth of cooperation of federal bureaus with each other and with state and other organizations. This cooperative spirit has been steadily promoted during the past few years and today has reached a stage never before witnessed. It is cooperation of this kind that has made possible the production of some of the reports here listed. For instance, in the preparation of *Water Supply Paper No. 838,* in paragraph V(s) mentioned, cooperation with the Geological Survey was necessary on the part of numerous offices of the Corps of Engineers, U. S. Army; the Mississippi River Commission; the Weather Bureau; the Soil Conservation Service; and the Tennessee Valley Authority. In addition, assistance in collecting data was rendered by individuals, corporations, organizations, and municipalities. The generous extent of collaboration and painstaking effort on the part of all these contributing agencies is particularly commendable. Unusual care was required by the Geological Survey staff in thus bringing together information from many sources, in correlating the data, and in effecting their presentation on a reasonably uniform basis.

During the year there was initiated in the federal service what is known as the "Cooperative Hydrologic Data-Gathering Program," by which the Weather Bureau is enabled to gather basic hydrologic data, principally relating to precipitation, needed by the Corps of Engineers and by the Bureau of Agricultural Economics in flood-control planning. This cooperative plan was made possible under the general authority of the Flood Control Act of June 22, 1936. The funds so made available to the Weather Bureau for the current fiscal year consist of $275,000 transferred from the War Department, and $100,000 from the Department of Agriculture funds. The data obtained will be multilithed for use. The operations of the Weather Bureau will be directed by its River and Flood Division and call for making the following additions to its present network of observation stations: Approximately 700 recording and 350 nonrecording rain gages east of the Continental Divide, and 20 batteries of snow gages in the western mountain areas, which number is planned to be further increased. The field observations will be made by technically trained personnel whose work will be supervised from 10 regional administrative offices and 15 suboffices scattered over all parts of the United States.

In the opinion of this Committee, this cooperative program is one of the most important strides taken toward overcoming the deplorable deficiency in rainfall and runoff data which, more than any other difficulty, has handicapped engineers engaged in planning flood control. The present cooperative program, taken in conjunction with such intensive data-gathering programs as have already been launched by the Miami Conservancy District, the Tennessee Valley Authority, the State of Pennsylvania, and other agencies, in their respective areas, is due, in time, to clarify the hydrologic aspects of flood runoff, and it is hoped will reduce the tendency toward formulating hydrologic theories based on scant data, which tend to confuse rather than assist engineers.

VII. Floods Caused by Ice

The Committee invites attention to the damage toll which ice in the rivers of the United States continues to take, and to the fact that no great progress has been or is being made toward its abatement. As recently as 1935, 1936, and 1938, serious damage from this source resulted in the eastern and northern United States. The year 1938 witnessed a 25-mile ice gorge in the Missouri River in South Dakota, another in the Mohawk River in New York State, and the destruction of the Falls View International Bridge and damage to power plants by an ice gorge at Niagara Falls; while thousands of families were made homeless by backwater from ice gorges in the rivers of Wisconsin, Michigan, and Illinois.

It is felt that this aspect of the flood problem has not received the consideration that is due it. Ice causes damage in two ways—indirectly, by backwater above gorges or jams, and, directly, by its formidable thrust. On a number of rivers, overflow due to ice jams has attained stages higher than any recorded by floods caused by storm runoff. Flooding caused by ice usually is more or less local in character and tends to recur at intervals down a valley, depending upon the manner in which an ice jam forms, breaks up, and forms anew at

points downstream. This aspect, and the fact that such overflows are believed by many to be unpredictable as to time or place of occurrence, have served to place ice jams and the damage caused by them in the category of "Acts of God" against which civilized man supposedly is powerless. The Committee is of the opinion that the entire subject of ice damage and its prevention possesses more tangible aspects than are commonly ascribed to it, and that, as a first step toward gaining a proper understanding of it, a careful review and analysis of the situation seems warranted.

Certain it is (despite the prevailing belief that winters are becoming less severe than formerly) that ice conditions in rivers continue to be as severe as in times past. The graphic accounts of ice damage along the Hudson, Delaware, Susquehanna, Potomac, and James rivers in 1936, 1918, and 1904 read much the same as do similar accounts published in 1861, 1846, 1816, and 1784. In only two respects has any progress been made in safeguarding the public—namely, through the more substantial construction of bridges and dams, and through the issue of Weather Bureau warnings. In other respects, the situation has not improved appreciably. The reasons for this are two-fold: The ever-increasing amount of damageable property that is being placed within the reach of ice and water; and the inefficacy of ordinary flood-control structures such as dikes, concrete walls, and storage reservoirs for combating heavy ice conditions. Such conditions, primarily, are a low-water phenomenon. Cold winter weather is not the agency that is solely responsible for the formation of ice jams. Next to freezing temperatures, the important agencies at work are the alternation of slow-moving water in the pools and swift water at the cross-overs, the respective velocities of which are inseparably connected with the formation of ice packs, gorges, and jams, coupled with fortuitous changes in the temperature of the air and of the water. A description of the manner in which these complex processes operate would be out of place here.

Dangers from backwater caused by ice jams are not confined to the lands immediately along the stream in which the ice gorges form. The backwater often extends up tributaries and carries river ice with it. When the tributaries so affected are frozen over, serious conditions arise, as their ice cover then is swept upstream—building floating ice mounds which imperil bridges and structures along the banks. Backwater from the famous 35-mile "Sugar Creek" ice gorge in the Ohio River, which lasted 58 days—from December 17, 1917, to February 12, 1918—pushed upstream two highway bridges located on tributaries and wrecked a third.

As a practical approach to a solution of the ice-flood problem, the Committee suggests that:

(1) All structures which public necessity demands be maintained in or along rivers, such as bridges, dams, hydro-electric plants, waterworks pumping stations, wharves, and navigation terminals, shall be built strong enough, or be provided with suitable protective devices, so as to be safe against destruction by ice.

(2) Zoning regulations be established requiring that all dwellings, hospitals, schools, public utilities, and damageable property be removed from and kept

out of the zone of ice blockades and backwater from ice gorges. This is damage prevention which not only saves large relief expenditures but obviates imposing annual maintenance and operation burdens on the taxpayer such as attach in perpetuity to flood-control works. This, furthermore, is in line with the provisions of Section 3 of the Flood Control Act of Congress, approved June 28, 1938, which provides for the evacuation of flood zones and for the rehabilitation of the persons so evacuated, when such action is deemed preferable to providing them with flood protection.

(3) Efforts be made to acquaint engineers and public authorities with the frequency with which destructive ice conditions have occurred in the past, and with the destruction that was so wrought, by issuing publications describing the historic ice floods on the principal rivers of the United States subject thereto. The practicability of preparing publications of this kind, as well as their usefulness, has been demonstrated by similar cataloguing of the great historic floods caused by storm runoff undertaken in recent years. Information of this kind could properly be gathered and published by a government bureau concerned with the water resources of the United States, and should logically be on file in the Inventory of Flood Data referred to elsewhere in this report.

(4) A review and analysis of damage by ice, its causative factors and possible remedies, be undertaken by a competent federal agency, with a view to pointing a way toward a constructive plan for eliminating this needless source of danger to life and property.

VIII. Acknowledgments

The Committee expresses its appreciation to Gail A. Hathaway, M. Am. Soc. C. E., principal engineer, Office of the Chief of Engineers, U. S. Army, for his courtesy in furnishing information relating to hydro-meteorological studies; also its appreciation of the many helpful discussions of its reports that have appeared in the *Proceedings* of the Society.

Respectfully submitted:

Robert F. Ewald
Frederick H. Fowler
Ivan E. Houk
C. W. Kutz
Charles W. Sherman
Gerard H. Matthes, *Chairman*
Committee on Flood-Protection Data

January 6, 1940.

AMERICAN SOCIETY OF CIVIL ENGINEERS
Founded November 5, 1852

REPORTS

OXYGEN CUTTING (FLAME CUTTING) OF STRUCTURAL STEEL[1]

PROGRESS REPORT OF THE COMMITTEE OF THE STRUCTURAL DIVISION

The Committee was organized in 1932 to ascertain the effect of usual oxygen-cutting processes on structural steel parts of various grades, shapes, and sizes. Almost from the outset, it was realized that satisfactory cutting of low-carbon, structural steel such as that of the American Society for Testing Materials, Specification A. S. T. M.—A7, presented no special problem. However, there were many difficulties due to apparently inconsistent behavior of alloy steels (usually coupled with increased carbon content) which required attention.

The first activity projected by the Committee was a survey of the literature on oxygen cutting. Following this it was expected that a fairly extensive series of tests would be required to confirm, modify, or extend the data developed by the survey.

Fortunately, the Committee was relieved of both these activities, since they were undertaken by organizations outside of the Society. Through the Welding Research Committee, sponsored by The Engineering Foundation, preliminary drafts of its comprehensive survey of the literature were placed at the Committee's disposal. This survey[2] disclosed the fact that so large a number of tests had already been made, both in the United States and in Europe, on the effects of oxygen cutting of structural grades of steel, that additional tests seemed to be unnecessary.

The essential data gathered by the Welding Research Committee's review of the literature on oxygen cutting may be summarized as follows:

The alteration of the properties of steel along an oxygen-cut edge has been shown by many competent investigators to be dependent on the metallurgical characteristics of the steel. The alteration is a normal result of the heating and rapid cooling which are inherent in the oxygen-cutting process, and it is particularly noticeable in steels that harden when cooled rapidly from high temperatures.

[1] Presented at the Meeting of the Division, January 19, 1940.
[2] *The Welding Journal*, Am. Welding Soc., July, 1939—supplement.

The accumulated evidence shows that there is no difficulty in simply cutting or severing any of the ordinary plain carbon or low-alloy structural steels. Low and medium carbon steels can be oxygen-cut without the occurrence of any undesirable conditions at the cut edge. Practically all investigators agree, however, that oxygen cutting of higher carbon and low-alloy structural steels may produce undesirable properties along the cut edge.

The metallurgical investigations of oxygen-cut steel show that there is always a heat-altered zone along the cut edge, the depth of which varies with the carbon and alloy content of the steel and with the thickness of the material cut. Heat-altered steel, in itself, even if it is somewhat harder than the base metal, is not necessarily undesirable. It is only when there is a band of extremely hard, brittle metal at the outer edge of the heat-altered zone that oxygen cutting can be said to have damaged the steel.

Tests of the mechanical properties of oxygen-cut steel have borne out the indications of the metallurgical studies in all respects. Bend, tensile, and hardness tests of oxygen-cut specimens have been thoroughly correlated and each of them is a good indicator of the existence of hardened constituents in the cut edge.

The studies of oxygen cutting of low and medium carbon steels show that they are not harmed by the cutting process. Metallurgical examinations show that the heat-altered zone is $\frac{1}{32}$ in. to $\frac{1}{16}$ in. deep in ordinary thicknesses of material and that the greatest hardness is about 70 Brinell above that of the base metal. Tensile tests of specimens oxygen-cut from these steels showed very slight increase in tensile and yield strengths, but practically no change in elongation and reduction of area over comparable machined specimens. Bend-test specimens can be bent 180° without failure. Impact tests showed no loss of strength due to oxygen cutting.

Investigations of oxygen-cut higher carbon and the low-alloy structural steels have shown that the heat-altered zone is similar in nature and extent to that of the low and medium carbon steels; but it may have a narrow martensitic band near its outer edge which is hard and brittle and likely to crack under stress. The occurrence of such hardness is not broadly predictable, but the composition of the steel, thickness of material, cutting speed, and cutting procedure each has an effect on it. Examination of oxygen cuts in various low-alloy steels shows that hardnesses of 500 Brinell may sometimes be developed if the cuts are made with no supplementary heat treatment. Pre-heating along the line of the cut and various methods of post-heating the heat-altered zone have been proved efficacious in preventing or eliminating the hardness which results from ordinary oxygen cutting of some of these steels. Tensile, bend, and hardness tests on specimens oxygen-cut from low-alloy steels have demonstrated that each will indicate the presence of any hardened zone at the cut edge. The bend test is probably more sensitive than the others, however, and it is much simpler and more foolproof in practical use for investigating the effects of any particular conditions of oxygen cutting of higher carbon or low-

alloy structural steels. In the case of steels that may be successfully oxygen-cut, there is no difficulty in welding on the cut edges if the shielded metal arc or gas welding process is used.

With the foregoing data available to the Committee, its task was no longer that of research, but, rather, had become one of interpretation. It was also quite obvious that many of the phenomena involved in the oxygen-cutting process are closely allied to, and in many cases indistinguishable from, those involved in welding. The analogous effects of the oxygen-cutting and welding processes on the base metal are apparent immediately, since both involve a relatively rapid heating and cooling cycle. Indeed, the most important factor, for any given steel, is the cooling rate of the steel.

The cooling rate may be changed or controlled, and the effect of those changes on structural grades of steel (such special grades as the stainless steels, for example, are not considered here) is as follows:

Factors that retard the cooling rate	Factors that accelerate the cooling rate
Pre-heating	Decreased initial temperature
Decreased speed of cutting	Increased speed of cutting
Post-heating	Increased thickness
Effect; lower hardness	Effect; higher hardness

These factors will always operate to produce the appropriate effect when any of them varies on a given grade of steel. However, it is important to consider also the effect of changes in chemical composition of steels, which also affect the hardness.

Plain carbon steels containing 0.35% carbon or less do not become very hard when heated and quickly cooled, and these steels usually may be successfully oxygen-cut without special precautions. As the carbon increases, or alloys are added, steels become more and more responsive to heat treatment—that is, they become harder when quickly cooled from high temperatures. Oxygen-cut edges in such steels are thus more likely to be brittle and subject to cracking. It is advisable, therefore, to retard the cooling rate of these steels in oxygen cutting. In fact, for carbon content over about 0.50%, this should always be the procedure.

The widespread introduction, in recent years, of a large group of low-alloy, high-strength structural steels is of considerable interest to engineers. Developed primarily with welding in mind, they contain such proportions of chromium, copper, manganese, molybdenum, nickel, phosphorus or silicon as will insure a relatively ductile product following the rapid cooling inherent in the welding process. The same characteristics, of course, are favorable to operations involving oxygen cutting.

A significant feature with respect to these high-strength, low-alloy steels may be stated thus: It does not necessarily follow that two different grades of steel, having the same tensile strength, can be oxygen-cut with equal satisfaction. As engineers know, similar differences in behavior have been found

to exist in the case of more usual fabricating operations, such as punching, drilling, planing, and milling.

During earlier stages of discussion, the Committee thought that its objective might be the preparation of a detailed specification that would set forth the conditions under which oxygen cutting could be performed without the necessity of subsequent processing of the oxygen-cut edge. However, upon considering the present state of knowledge, and the rate at which new grades of steel are being developed and introduced upon the market, it did not appear to be possible, at this time, to issue a satisfactory specification. Furthermore, the Committee recognizes that the Society is not primarily a specification producing body. Therefore, it has abandoned the plan of preparing such a specification; rather, it presents herein what it considers to be criteria for the safe use of the oxygen-cutting process on structural steels. Such criteria, it is believed, may assist others to write specifications.

Conclusions

The conclusions of the Committee, based upon its study of the aforementioned research reports, and upon the practical experiences of its members in the use of the process, are as follows:

(1) Oxygen cutting of structural steel inherently involves a heating and quenching cycle and therefore produces some hardening of the cut edge. The amount and severity of this hardening depend primarily on the composition of the steel and to a lesser extent on its thickness and the cutting procedure. Oxygen cutting may or may not, depending upon the procedure, lower the stress-resisting properties of the cut edge through the "stress raiser" action of roughnesses along that edge. This hardening or roughening which may result from oxygen cutting may make the cut edge unacceptable for some uses although under other circumstances it may be quite acceptable.

(2) The acceptability of an oxygen-cut edge should be determined by tests that compare its properties with those of the parent metal. There is ample evidence to show that the behavior of an oxygen-cut edge in bend tests will indicate, adequately, whether the mechanical properties of the steel have been lowered by the cutting procedure used.

(3) Oxygen-cut edges should possess mechanical properties equal to those required by the specifications governing the structure.

(4) The Committee believes that adequate evidence has been presented to demonstrate that ordinary grades of carbon-steel structural quality, such as that defined by Specification A. S. T. M.—A7, conform to the foregoing criteria as follows: If the oxygen-cutting torch is guided by mechanical means, the cut edges will give tests equal to planed edges; if the torch is guided by hand and any rough edges are afterwards made smooth by chipping or grinding, the cut edge will give tests at least equal to those obtained by shearing.

(5) The Committee believes that the statements in Conclusion (4) have not been shown to be sufficiently true of the low-alloy, high-strength structural

steels, and that such steels may require special cutting procedures and treatment if the mechanical properties of the cut edge are to be equal to those of the parent metal. Any oxygen-cutting procedure applied to low-alloy, high-strength structural steels should be proved adequate by means of bend tests of the cut edge before its use is approved as a fabrication process. Edges that are not significantly stressed (as for example the edges of base plates) may be excepted from this requirement.

(6) Conclusions (2) to (5) are set up to conform to present structural specifications which are based on static conditions. Fatigue tests have not yet become a part of such specifications, and too few data are available to permit conclusions to be drawn concerning the effect of oxygen cutting on fatigue properties.

Acknowledgments

The Committee here records its sincere appreciation to the Welding Research Committee for its cooperation in making available preliminary drafts of its "survey of the literature." It further desires to express its regret at the resignation of its Chairman, Fred T. Llewellyn, M. Am. Soc. C. E., caused by his retirement, in June, 1939, from business activities.

Respectfully submitted:

J. H Critchett	Jonathan Jones	N. H. Orr
L. H. Davis	W. J. Krefeld	H. T. Welty
P. J. Freeman	J. R. Lambert	W. M. Wilson
C. F. Goodrich	L. E. Moore	Milton Male,
L. R. Hjorth	J. C. Ogden	*Chairman*

Committee of the Structural Division on Oxygen Cutting of Structural Steel

February 15, 1940

AMERICAN SOCIETY OF CIVIL ENGINEERS

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DISCUSSIONS

SETTLEMENT STUDIES OF STRUCTURES IN EGYPT

Discussion

By Gregory P. Tschebotareff, M. Am. Soc. C. E.

Gregory P. Tschebotareff,[22] M. Am. Soc. C. E. (by letter)[22a]—The contributions made to the problem of settlement studies by the discussers of the paper are appreciated sincerely. The writer is glad to have the opportunity to clarify the problems discussed by stressing some of the data contained in the plans and diagrams, which were overlooked by the discussers, and by supplying additional data concerning the studies reported in the paper in a necessarily condensed and summarized form.

The Design of Structures to Withstand Differential Settlements Is Quite Practicable.—It was gratifying to note that Mr. Wilcoxen recommended wide recognition of the writer's chief conclusion concerning the necessity for settlement studies in all large cities. Mr. Wilcoxen suggests that two methods may be "developed" for the purpose of preventing detrimental effects of differential settlements.

The first of these two methods—that is, a suitable preventive design of the structure—is quite practicable at present. However, it does not always imply merely the provision of sufficient rigidity for the combined superstructure and foundation. In some special cases (such as steel tank foundations) a flexible foundation, matching the flexible superstructure, will give better results.[23]

Further, a suitable architectural design is important for all buildings on soft ground. The omission of high towers and low annexes, and the provision of deep basements, as well as other measures permitting the use of reinforced concrete walls several stories high, are among the features that help to equalize settlements.

Note.—This paper by Gregory P. Tschebotareff, M. Am. Soc. C. E., was published in October, 1938, *Proceedings*. Discussion on this paper has appeared in *Proceedings* as follows: February, 1939, by Messrs. L. C. Wilcoxen, Trent R. Dames, and Edwin J. Beugler; May, 1939, by W. S. Hanna, Esq.; and June, 1939, by D. P. Krynine, M. Am. Soc. C. E.

[22] Asst. Prof., Civ. Eng., School of Eng., Princeton Univ., Princeton, N. J.; formerly, Research Engr., Foundation Soils Research Laboratory, Egyptian Univ., Cairo, Egypt.

[22a] Received by the Secretary January 10, 1940.

[23] "Progress Report of Special Committee on Earths and Foundations," *Proceedings*, Am. Soc. C. E., May, 1933, p. 812.

The construction procedure followed may also be of importance. For instance, if a low annex is essential, settlement stresses in its framework may be reduced considerably by postponing the erection of the annex as long as possible. Its simultaneous construction with the upper, and not the lower, stories of the adjoining high part of the building may suffice in the numerous cases when a large percentage of the final settlement will have occurred during construction.

The practical importance of the knowledge of the settlements to be expected is not limited to cases of excessive settlement. Any definite knowledge in the matter reduces guesswork and permits more economical and rational designs.

"Mudjacking" of Compressible Soils for the Purpose of Increasing Their Resistance Is Impracticable.—Mr. Wilcoxen further suggests treating foundation soils to prevent differential settlements by using "mudjacking" pipes extended deep into the foundation soil.

This suggestion does not appear at all promising. "Mudjacking," as successfully used in highway construction, serves to raise sunken pavements by injecting, under pressure, a soil-water slurry between the relatively light concrete pavement slab and the surface of the soil. The entire operation is similar in principle to the frequent and successful use of jacks for the purpose of raising a bridge bearing and then wedging it up the distance that the pier beneath it had settled.

The problem is different, however, if one tries to inject anything into the ground itself for the purpose of preventing its compression or deformation under load. The injected liquid tends to flow in the direction of least resistance. This circumstance may be utilized to fill, with cement grout, or chemicals, cracks in rocks or in dry clays, or cavities beneath foundations in order to prevent seepage. In plastic materials, and even in sands, this very circumstance means that only some irregularly distributed and more permeable, but not necessarily more compressible, veins of the soil may be given rock-like hardness. Obviously, this is of no advantage, and may be directly detrimental where differential settlements are concerned.

In 1932 a European firm specializing in this kind of work attempted to consolidate the weak, dark clay beneath the pile bearings of Building *T* by successively injecting into that clay separate solutions of two different chemicals. This attempt failed, although the two solutions formed a "gel" when coming into contact with each other and gave a rock-like hardness to that clay when mechanically mixed with it in the laboratory. A further test was made with the chemical-consolidation method in a dry, silty sand layer. A subsequent excavation of an injection revealed what resembled an inverted sandstone tree. The trunk was only about 10 in. in diameter. Four or five branches stretched out horizontally, one of the branches being almost 5 ft long. Therefore, in order to create a continuous mass of solidified, silty sand, the injection pipes would have to be driven at not more than 10 in., center to center, and much closer in less pervious soils. This is obviously impracticable in most cases.

The General Principles of Soil Mechanics Are Valid in Any Locality.—Mr. Dames considers the author's conclusion that "Results obtained from observations in one locality are not necessarily valid in other localities" to be the most

significant single statement of the paper. The writer agrees that this conclusion is very important and should be emphasized. At the same time he wishes, emphatically, to warn against the inference contained in further statements by Mr. Dames that most of the new soil mechanics theories (unspecified) are seldom valid except for glacial clays of Northeastern United States and Europe.

The general principles of soil mechanics are valid everywhere. It is a frequent but regrettable practice to apply a theory, superficially, to local conditions which do not correspond to its underlying assumptions. After the inevitable failure the entire theory is then often proclaimed invalid without further analysis. The writer cannot judge the nature of the "different" soil tests and methods advocated for Southwestern United States by Mr. Dames since their nature is not specified; but whatever their nature or their merits they cannot warrent arbitrarily ignoring conclusions based on a theoretical analysis of observations. This is especially true when the observations are made under field conditions closely approximating the assumptions on which a widely known theory[24] is based, whereby both the field and the laboratory results agree as well as agreement can be expected, theoretically. To illustrate these remarks the writer will examine some of the discussions of the time-settlement curves, which ignored the effect of the permeability of a stressed clay layer on the rate of its compression.

The Rate of Settlement of a Fully Saturated Clay Layer Is Governed Primarily by Its Permeability and Thickness.—In discussing Building IV, Figs. 5 and 6, Mr. Dames states: "* * * the continued, undiminished rate of settlement since the middle of 1936 could be interpreted as caused, at least in part, by shearing failure and the resulting plastic flow of over-loaded soil from beneath the foundations of this building." An examination of Fig. 6 shows that the rate of settlement had not been constant since the middle of 1936. Seasonal variations of subsoil water levels provide definite breaks in the time-settlement curve; the rate of settlement is slowest when the subsoil water level is highest, almost reaching to the elevation of the foundations; then the rate of settlement increases as the subsoil water level drops about 14 ft. The reverse should have been the case if Mr. Dames' interpretation were correct, because a decrease in the moisture of a cohesive clay increases its cohesion and therefore its shearing strength, simultaneously decreasing its tendency to undergo plastic deformations. In the case of Building IV the borings were made at the time of lowest water level and the chart of laboratory test results (Fig. 6) shows that at that time the clay soil immediately beneath the foundation was very stiff and was of a definitely brittle type least likely to undergo plastic deformations.

Thus all available facts contradict the aforementioned interpretation by Mr. Dames. On the other hand, computations based on the rate of compression of the deeper lying, soft, dark clay layer following the expulsion of surplus moisture show that settlements of Building IV due to that cause were still to be expected during 1936–1937. This therefore provides reasonable explanation for them. Nevertheless, the writer does not deny that plastic shearing deformations may

[24] "Factors Affecting the Accuracy of Settlement Rate Forecasts," by Gregory P. Tschebotareff, *Proceedings*, 18th Annual Meeting, Highway Research Board, Part II, December, 1938.

have some influence on the rate of settlement and has recognized this fact in the past.[24] On deep lying, fully water-logged, clay soils, however, this cause is likely to be of any relative importance only after the settlements due to the expulsion of surplus moisture have slowed down almost entirely. The results of further-continued observations may disclose whether this is the case for any of the buildings under discussion. The only case reported in this paper where plastic shearing deformations may be at all reasonably assumed to be of importance thus far is Building VI (see Fig. 8). The writer stated that the cause of the continued settlement was not clear and suggested tentatively that it might be attributed to the effect of gradual consolidation of the overlying silt layers and to the resulting decrease in the frictional support they temporarily provided. Mr. Dames misunderstood this to mean that the silt layer was believed to be consolidating under its own weight. Undoubtedly this is most unlikely. The writer referred to consolidation under pressures from the building transmitted by the footings which capped the piles and by the relatively high skin friction of the piles themselves.

The writer agrees that gradual plastic shearing deformations of both the silt and the sand (as suggested by Mr. Dames) may have been equally or even more important than consolidation in this one case of Building VI. Furthermore, the possibility of some small, plastic, shearing deformations is not entirely excluded in the cases of Buildings II and III, but a much more likely explanation is provided by the action of the repeated variations of the live load. The total pressure on the ground surface beneath Building II is decreased by 20% and then increased by the same amount twice every week. A similar but daily fluctuation of about 10% of the total pressure on the ground surface occurred beneath Building III in the zone of Points 26 and 28 under the balcony of that movie house. It was only these points which showed any noticeable continuation of the settlements, whereas Point 1, with the same unit pressure on the surface of the ground, but so located that the effect of the live load variations would be relatively small, showed only a negligible continued settlement of about $\frac{1}{16}$ in. during 1.5 years.

Mr. Dames mentions vibrations as a possible cause for continued settlements; but they could not have been of importance in this case. No machinery was present. None of the lifts of Building *T* functioned until 1934, none of them was located in the zone of continued greatest settlements, and no cracks were to be found near them. Street-car lines and an otherwise busy thoroughfare passed next to Building *T*; but this thoroughfare ran east and west along the north side of the building, which side settled slightly less than the south side of the building (Fig. 13) where it adjoined a small street with much less traffic and no street-car lines. The reverse should have been the case if traffic vibrations were influencing the settlements.

Mr. Beugler states that: "From the time-settlement curves for the various buildings it may be concluded that over-loading is responsible for most of the progressive settlement." This statement appears to be due to his belief that settlement "due to compression, shrinkage, and more or less elastic reaction, * * * ceases when the building is finished, or shortly thereafter, provided the unit loading of the soil has been selected conservatively."

It can be demonstrated easily in any soil mechanics laboratory that such belief is entirely without foundation, in so far as the compression of clay soils is concerned. The compression of a fully saturated, confined, clay sample never occurs instantaneously after application of the load, no matter how small the load increment is; and it can be extremely slow for very impermeable clays. Clays and silts from the northeastern, southern, and southwestern regions of the United States have been tested by the writer at Princeton University and have not shown any behavior, in this respect, different from the soils of Central Europe or Northern Africa. In all cases the permeability of a saturated clay exercised the most important influence on the rate of its compression. The relative influence of other factors may vary and will increase for more pervious or only partly saturated soils; but for fully saturated clays this influence always remained secondary as compared to the permeability of such clays.

An illustration of the contradictions in the aforementioned reasoning of Mr. Beugler is provided by Building IX (Fig. 11) which exerted a unit pressure of only 0.4 tons per sq ft on the surface of the ground. This pressure cannot be called anything but a conservative value for any soil, and it could not have been reduced further for the continuous mat of the building. The phenomenon of continued and gradually decreasing settlement of this building is fully explained by the gradual expulsion of water from the compressed voids of the silt and clay beneath the building, whereas "overloading" certainly does not offer any explanation. The settlements of this case almost ceased a year after the end of the construction, as should be expected because of the relatively thin compressible layer (20 ft). With a thicker layer they might have lasted much longer.

Soil Exploration for Foundation Work and Soil Testing Prodceure for Engineering Classification Purposes.—Mr. Dames questions the value of the plastic and liquid limit tests when they are related to foundation soils in the undisturbed state, but overlooks the fact that nowhere in the paper were the plastic and liquid limits reported alone. In all cases a curve for each of these limits was plotted along the entire depth of the boring, side by side with a curve for the natural water content (in the undisturbed state). The curves for the compressive strength and the strains at failure, both in the undisturbed and in the remolded state, were plotted also. Any one of these tests is inadequate, if taken by itself; but it supplements the others so that, taken together, they give a good idea of the undisturbed nature of the soil profile.

The writer has a strong distrust of the sufficiency of descriptions of "appearance, texture, and local behavior," as suggested by Mr. Dames, unless they are accompanied by reliable data fixing the definite soil properties numerically. Such terms as "hard" and "soft" or "plastic" and "semi-plastic" may mean entirely different things to different people.

Shear and consolidation tests are undoubtedly among the most important, but they require so much time that in most cases it is not possible to perform them as routine classification and exploration tests. They then must be performed on a few selected samples only, as was done in Egypt. It should be mentioned here that, tested after complete disturbance, followed by full consolidation, at a rate of application of the shearing force equal to one-twentieth

of the normal load per minute, both the dark and the brown clay had approximately the same value of the angle of internal friction (20 degrees). Only the cohesion varied.

The unconfined compressive-strength test on undisturbed samples extracted from borings was chosen as a routine test because it gives a direct measure of the cohesion of the soil and therefore of its shearing strength in the undisturbed state; at the same time, it can be performed much more rapidly than a properly conducted shear test. The strain at failure was given because it showed whether the undisturbed material was plastic (20% strain being taken as failure by plastic flow), semi-plastic, or brittle. The natural water content, given together with the plastic and the liquid limit, also illustrates the state of plasticity of the undisturbed soil.

In itself, an indication of the compressive strength in the undisturbed state is insufficient to identify a soil unless supplemented by an indication of the clay content or of the liquid limit. If the compressive strength has a low value, the liquid limit serves to show whether this is due to a low clay and silt content and to the resulting low cohesion (low liquid limit and low natural water content) or to an unconsolidated state of the undisturbed clay material (high liquid limit and high natural water content).

Some modifications of this method of soil exploration may naturally become desirable when dealing with other soil conditions. For instance, for soils that are permanently above the ground-water table, the indication of the volume, in place, may become desirable in addition to the water content. For sands and sandy silts, all laboratory tests could be replaced, usefully, by indications of the number of standard blows per foot required to force down a standard sampling spoon.

The method which the writer developed for work in Egypt is particularly adapted to conditions similar to the ones encountered there—that is, for the exploration of foundation conditions on cohesive soils beneath subsoil water level. Its practical advantages for such soil conditions appear to be recognized in the United States, since a similar method was used for the classification of soils along the Chicago subway.[25]

Limitations in the Value of Field Load Test.—Mr. Dames considers that the data given by the writer is not sufficient to "close the case" and to condemn load tests. No indiscriminate condemnation of such tests was contained in the paper; but, in suggesting load tests "not necessarily at foundation depth," Mr. Dames overlooks the fact that load tests are more unreliable than ever at elevations below subsoil water level. The excavation of a sheeted test pit to reach the layers likely to provide the "seat of settlement" (for instance, the dark clay) would have been entirely impracticable for the cases reported. It may be seen from Figs. 2, 4, 6, and 10 that the natural water content of the dark clay approaches very closely, and sometimes even exceeds, the water content at the liquid limit. When a boring was stopped for the night in a layer of dark clay or silt, one would very often find, in the morning, that it had been squeezed 2 or 3 ft up inside of the 6-in. steel casing of the bore hole. This would

[25] "Soil Tests Check Chicago Subway Work," by Ralph B. Peck, Jun. Am. Soc. C. E., *Engineering News-Record*, December 7, 1939, Fig. 1.

happen despite the fact that the casing had been kept filled with water to the very top in order to more than counterbalance any possible hydrostatic uplift pressure on the soil at the bottom of the bore-hole casing. The squeezing, therefore, was caused only by the weight of the overlying soil layers and of buildings in the vicinity. Short of using pneumatic caissons, in most cases there would be no way to make a load test on the dark clay in its really undisturbed state. Because of their high cost, pneumatic caissons have been used in Egypt only for some of the largest bridges.

Mr. Dames advocates tests on footings of different sizes. Several semi-empirical theories have been developed for the purpose of estimating the settlement of a full-sized structure from data supplied by several test footings of different sizes. However, such estimates may be successful only when all the compressible layers can be reached by excavation and tested, and when each of these layers is homogeneous, horizontally, beneath the entire area occupied by the building. The numerous records from different localities examined by the writer tend to show that a naturally deposited soil layer with such a uniform degree of compressibility may be more in the nature of an exception than a rule and that non-uniform soil deposits near the surface are at least equally frequent.

Fig. 16 illustrates an extreme case in point and gives the results of load tests made on a weak surface layer, presumably an old fill, near Building *T*. Complete stabilization after each load increment was permitted, with at least 24 hr elapsing between the increments; the settlements were measured to about 1.0 mm (= 0.04-in.) accuracy, which is more than sufficient for all practical purposes. The writer wishes to emphasize that settlements cannot be measured in the open field to the accuracy of 0.001 in., as suggested by Mr. Dames. It is possible to provide dial gages reading to such accuracy, but thermal deformations of the necessarily long cantilevered supports, as well as other factors, preclude an actual accuracy exceeding 0.01 in. when observations are made over a period of several days.

It may be seen from Fig. 16 that the results would have been very embarrassing to any one who intended to use them for any estimates of the type suggested by Mr. Dames. The footing (10.8 sq ft) settled slightly more than the footing that was four times larger (43.2 sq ft). The reverse relation of the settlements should have been observed in homogeneous soil.

There was no opportunity for the advantageous use of soil load tests in any of the buildings described in the paper. Such tests can be applied only for testing surface layers above subsoil water level, thereby providing information that is seldom even of a limited practical value. In the few cases when it does have some practical value, the writer believes it preferable not to use footings of different sizes, but to make more numerous tests over the entire area of the future building with a small footing of some standard size—say, 1 sq ft. The results could then be compared, both individually and as averages, to a set of load-settlement curves for a footing of that same standard size, typical of the settlement of very hard and of very soft soils, and of intermediate types.

Effect of Excavation.—Mr. Beugler agrees with the writer that no decrease of settlement was observed as a result of excavation and states that this was doubtless true for the case cited (Building II). He then cites another case in

which settlements were reduced by excavation after construction. This is quite understandable, since such excavation definitely reduces the pressures on compressible layers without allowing any dislocation of their structure. The writer has also reported a similar case.[26]

If the excavation is done before construction, however, everything will depend upon whether the deeper lying soil is dislocated as a result of the excavation. In the case of naturally stiff and impervious clays, any appreciable

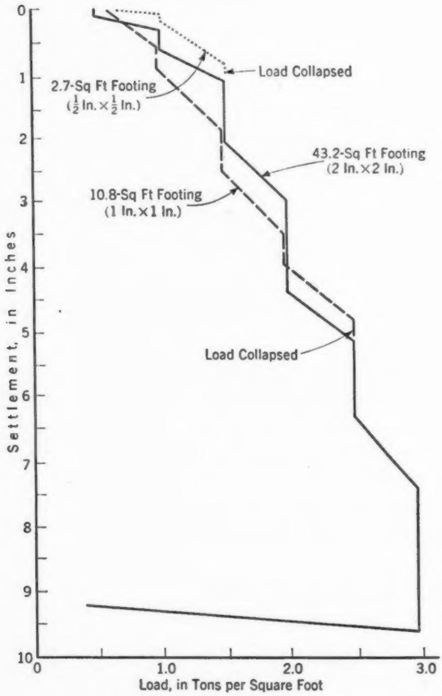

FIG. 16.—RESULTS OF LOAD TESTS ON WEAK LAYER OF IRREGULAR COMPRESSIBILITY

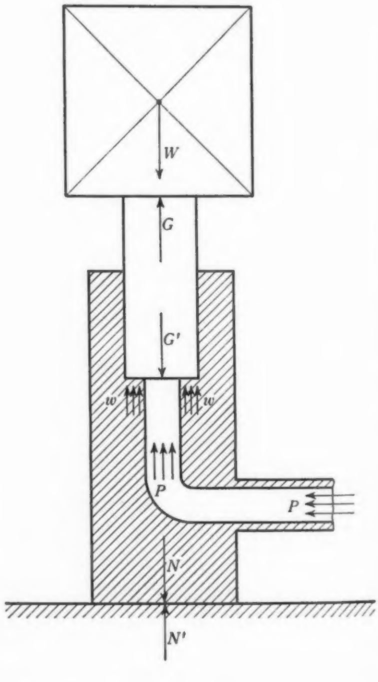

FIG. 17.—MECHANICAL EQUIVALENT OF INCORRECT ASSUMPTION CONCERNING THE SWELLING OF SOILS

swelling may sometimes be prevented by draining the foundation pit properly and by rapid construction. The observations made under similar conditions at the Telephone Building at Albany, N. Y., led, at first, to unrestricted recommendations concerning the utility of excavation as the only reliable method for reducing settlements on clays.[27] Later observations disclosed limitations of the method in the case of weak plastic clays squeezed into the foundation pit. In extreme cases (Mexico, D. F., Mexico) the recorded rise of the bottom of the

[26] "Settlement Studies of Structures," by Gregory P. Tschebotareff, *Civil Engineering*, November, 1938, p. 751.

[27] "The Structure of Clay and Its Importance in Foundation Engineering," by A. Casagrande, Assoc. M. Am. Soc. C. E., *Journal*, Boston Soc. of Civ. Engrs., April, 1932, Fig. 28, pp. 191–193, and 203.

pit sometimes reached 4 ft.[28] Special measures must then be adopted to counteract this rise and the accompanying weakening of the surrounding clay.[29]

Observations on Building II have disclosed a further limitation when fairly stiff swelling clays and silts are permitted to absorb moisture after removal of the over-burden. No drainage could have helped here since excavation was extended to the lowest water level; furthermore, the foundation pit was left open for some time.

Mr. Dames states that "no data are introduced to disprove the logical premise that this structure would have settled more than 1 in. if founded at, or near, the surface of the ground." In discussing Building II, the writer referred definitely to Fig. 2 and stated that, contrary to what should have been expected, settlements were registered immediately after the loads were applied to the soil. If the effect of excavation had been at all favorable, the rate of settlement should have been either equal to zero until the weight of the building had reached the weight of the excavation or, at least, it should have been slower as compared to the rate of later settlement. An examination of Fig. 2 shows that exactly the reverse is the case. The rate of settlement was particularly rapid at the beginning of the construction. After the weight of the building exceeded the weight of the excavation, the settlement rate decreased almost three times during the next, and last, four months of construction.

Mr. Dames justifies his point of view by comparisons with other buildings, all of which are based on arbitrary and erroneous premises. For example, he states that "Building III is on spread footings and is not comparable as to load over an extended area" (Building II). An examination of Figs. 2, 3, and 4 shows, first, that the loaded areas of both buildings are not much different in size. Then, Building III does not rest on isolated spread footings but on continuous strip footings which merge into a raft covering a surface shaped like a hollow rectangle. Furthermore, three sides of the hollow, rectangular-shaped mat of Building III are surrounded by extensive fill which, in addition, covers an 8-ft fringe of the mat itself (Fig. 3). The additional pressure on the surface of the ground due to that fill equals 38% of the total load of the building (Fig. 4). For these reasons, the average pressures beneath Points 26 and 28 of Building III (Fig. 3(*a*)) as computed by the writer along the entire depth of the compressible layers above the sand are slightly greater than the corresponding pressures beneath Building II, even if these latter pressures are computed under the assumption that they have not been reduced by excavation. The type of soil formation is similar beneath both buildings. The thickness of the stressed compressible soil layers is slightly greater in the case of Building III (27 ft) than in the case of Building II (22 ft); nevertheless, over the same period of three years Building III has settled slightly less (20 mm) than Building II (22 mm). The comparison between these two buildings, therefore, is entirely possible and fully substantiates the statement of the writer that the excavation did not decrease the settlements of Building II.

[28] "Discussion of the Movements Within Foundation Pits During Construction," by J. A. Cuevas, Lazarus White, M. Am. Soc. C. E., and Karl Terzaghi, M. Am. Soc. C. E., *Proceedings*, International Conference on Soil Mechanics and Foundation Engineering, Vol. III, *Paper N-15*.

[29] "Modern Methods of Design and Construction of Foundations," by Lazarus White, *loc. cit.*, *Paper N-14*.

Buildings II and IV cannot be compared as suggested by Mr. Dames because his statement, "A similar sequence of soils both as to classification and description by the physical tests given exists under both of Sites II and IV," is incorrect. He overlooked the fact that beneath Building IV the dark clay was five times thicker than under Building II and later made an incorrect assumption concerning the compressibility of the dark clay.

Compressibility of Soils.—Mr. Dames assumed the dark clay of Building *T* to be just as compressible as the brown clay above it, whereas in all the cases reported in the paper the brown clay formed, on an average, a much more compact and stiffer material than the dark clay. This can be seen by examining Building IV in Fig. 6 where this difference was particularly pronounced. The brown clay had a natural water content of about 25% and a compressive strength varying from 1.3 to 3.2 tons per sq ft, whereas the deeper lying dark clay had a greater natural water content varying from 35% to 40% and a smaller compressive strength of about 0.7 ton per sq ft.

As stated in the paper, laterally confined, combined compressibility and permeability tests were also made on both types of clays and served to interpret the field observations. About forty such tests were actually performed. The "*X*" coefficients for these laboratory compressibility tests, performed in the usual standard manner, varied from 0.012 to 0.018 for different brown clays and from 0.024 to 0.045 for different dark clays.

Building I provided a key for an attempt to interpret and correlate the results of some of the field observations and laboratory tests. This was possible because the settlements of that building were caused almost entirely by the compression of a stiff brown clay layer (Fig. 1). The writer found the value of $X = 0.007$ to be approximately correct for the very stiff brown clay beneath Building I.[3] This value may be considered as not being too high for the still stiffer and more compact brown clay immediately beneath one of the wings of Building IV (Fig. 6). The compressibility of the compact brown silt beneath the rest of that building cannot exceed, to any extent, that of the brown clay. This follows from a comparison of the symmetrical computed lines of equal pressure and of the very slightly unsymmetrical lines of observed equal settlements (Fig. 5). The slightly greater settlement of the right wing of Building IV is directly proportional to the slightly greater depth of the compressible layers there.

Taking a value of $X = 0.007$ for the upper 18 ft of brown clay and silt, with an average pressure of 0.6 ton per sq ft along their depth, one obtains a value for their compression equal to: $0.007 \times 0.6 \times 18 \times 12 = 0.90$ in. Thus far, the total settlement has been reported as slightly exceeding 3 in. Thus, at least 2.1 in. of settlement must have been caused by the compression of the dark clay and silt. The dark silt is much less compressible than the dark clay, as demonstrated by tests, and a coefficient of $X = 0.007$ similar to that of the upper brown silt represents a liberal value for it. This gives a total compression of the 6-ft layer of dark silt equal to: $0.007 \times 0.5 \times 6 \times 12 = 0.25$ in. and leaves $2.10 - 0.25 = 1.85$ in. compression for the 10-ft dark clay layer. This

[3] *Proceedings*, International Conference on Soil Mechanics and Foundation Engineering, *Paper D-1*, Vol. 1, 1936.

figure, in turn, leads to the following average value of the compressibility coefficient for that dark clay: $X = \frac{1.85}{0.5 \times 10 \times 12} = 0.031$, which agrees well with laboratory test values for dark clays from different localities (ranging from 0.024 to 0.045). The clay is of the non-swelling type so that disturbances due to this cause cannot be considerable. Therefore, the value of $X = 0.030$ was reported as representing an average lower limit approximately determined from observations; the writer considers it to be reasonable. The approximations involved should not produce a cumulative error of much more than 50%; greater accuracies can never be reached for this type of work except accidentally.

The upper limit of $X = 0.070$ (a minimum average value) was reported for the dark clay on the basis of the field observations of Building IX only. The extremely high soil compressibility in this one case was very puzzling to the writer at first, until the examination of an old map made by the surveyors of Napoleon revealed that in 1798 the main channel of the Nile River flowed through what is now the site of Building IX. This gave a reasonable explanation for the abnormal compressibility since the river did not change its course until about 1850 when the present site was silted rapidly. The resulting soil formation, although covered by a relatively thin, dry, and hard crust, may be likened, therefore, to a hydraulic fill rapidly deposited over a horizontal surface of large area. Instances of the high compressibility of such fills have come to the notice of the writer.

The foregoing explanation appeared all the more reasonable since a further study of maps, based on medieval data,[30] showed that the sites of all other buildings were much more ancient and that the main channels of the Nile River near Cairo, Egypt, had not undergone any other appreciable changes since the Fourteenth Century.

Professor Hanna points out that in addition to swelling there must be also other sources of error, such as plastic deformations during sampling and friction along the sides of the testing ring. The writer has always fully endorsed similar views. Professor Hanna mentions some new studies he is undertaking and reports that "Preliminary results of these studies indicate that the author's statement that the method described in the paper has proved a success cannot be considered final." No such statement is contained in the paper; nor is it implied directly or indirectly. It would be entirely contrary to the views of the writer to suggest that any one method could take care of the numerous possible variables affecting the engineering properties in general, and the compressibility in particular, of different types of soils. The writer merely reported a method which disclosed the relatively great importance of the swelling of some compact clays as a source of error; this method also gave a procedure to counteract this swelling which checked with the field observations. Far from recommending the unrestricted use of this procedure, the writer presented a special discussion to the International Conference on Soil Mechanics and Foundation Engineering in 1936[31] for the purpose of stressing, on the basis of the observa-

[30] "Cairo—Origin and Development," by C. R. Haswell, *Bulletin* de la Société Royale de Géographie d'Egypte, Tome XI, Decembre, 1922, Plate II.

[31] *Proceedings*, International Conference on Soil Mechanics and Foundation Engineering, Vol. III, *Discussion No. D-19.*

tions of Building IV, the limitations when applied to non-swelling clays of the method described earlier.[3] In the present paper the writer has again stressed the limitations of this and other methods of settlement forecast and has emphasized the fact that they depended on the type of soil and on the method of sample extraction.

Pressure Distribution Computations.—Professor Krynine has stressed an mportant point by emphasizing the fact that the Boussinesq formula cannot be applied, very well, for estimating vertical pressures on deep lying compressible layers if these pressures are transmitted to the layers through an overlying, more rigid layer on which the foundation rests. In the writer's opinion, this "arching effect" of the upper layers, which relieves the pressures on the lower, weaker layers by distributing them over a greater area, should decrease in importance (1) with a decrease in the difference of the shearing rigidity of the two layers, (2) with a decrease in the thickness of the upper layer, and (3) with an increase of the area of the building. Therefore, in the case of the Egyptian buildings reported, this factor cannot have been very important. Any considerable excess in the assumed values of the actual pressures would mean that the assumed "X" values for the stiff brown clay ($X = 0.007$) would have to be increased and those of the dark clay ($X = 0.030$) decreased.

With reference to Fig. 15, the writer agrees that in some cases the average pressure in the ground may be neither at one-half nor at two-thirds of the depth of the compressible layer, but may have an intermediate value. However, for the ratio between the depth of the compressible layers and the foundation area of the cases reported, the foregoing assumptions do not involve an error exceeding 10%,[32] which is negligible as compared with the possible degree of accuracy of the settlement analyses as a whole.

Settlement of Pile Foundations.—Arbitrarily, Mr. Dames assumed that the compressibility of the dark clay is equal to that of the stiff brown clay ($X = 0.007$) and expressed the "belief that the settlement of Building *T* has been largely the result of individual pile failure and not due to the compression of the 10 ft of dark clay between the pile points and the sand."

Under the heading "Compressibility of Soils" herein the writer has explained that the foregoing assumption is entirely unjustified and that the estimated average compressibility of the dark clay under Building IV was $X = 0.030$. Earlier laboratory tests performed by the late Professor Kögler at Freiberg on soils from Building *T* had given approximate values of $X = 0.017$ for the brown clay and $X = 0.026$ for the dark clay.

Computations similar to the one performed for Building IV give the values of 5.3 in., 1.0 in., and 0.5 in. for the respective compressions of the dark clay, the brown clay, and the silty sand. Since the total observed settlement is 10.0 in., this leaves only 3.2 in. to be attributed either to too small values of X (taken at 0.026, 0.007, and 0.007 respectively) or to additional effects of plastic shearing deformations reaching to some distance around the piles. Such shearing deformations are likely to be relatively important only along the outside perimeter of the building. The term "group penetration" used by Mr.

[32] *Proceedings*, International Conference on Soil Mechanics and Foundation Engineering, *Paper No. E-1*, Fig. 18, pp. 57 and 58.

Dames is misleading since it implies the formation of a definite, vertical, shearing surface of failure in the soil around the pile groups and their "slipping" past the surrounding ground, together with the footings which cap them. No evidence of any such "slipping" could be obtained in this building. The very argument, correctly used by Mr. Dames to explain larger "group penetration" as compared to smaller single pile penetration, testifies against this conclusion in this case. The pile groups are so close to each other in the eastern part of the building that the shearing resistance between the stiff brown clay prisms confined by these groups and the piles themselves should be of the same order of dimension as the resistance to compression of the weaker dark clay supporting these prisms. Thus the piles supporting the building, on the whole, must have settled with the surrounding soil, except possibly along the outside perimeter of the building; and most of the surface settlement must have been caused by the compression of the lower lying dark clay layers.

To study this question further, in 1931 the writer introduced into the soil beneath Building *T* nine underground bench-marks almost identical to a type designed by Professor Terzaghi.[33, 34] They were placed in three groups at three different elevations. The results were satisfactory in a qualitative sense, showing that a large part of the settlement was due to the compression of the layers beneath the pile bearings, perhaps even including, to a slight extent, the deeper sand layers which have been assumed incompressible in this paper. However, the results from the separate bench-marks were unsatisfactory in a quantitative sense, since they were not in a logical numerical agreement with each other. It is impossible to say whether these numerical discrepancies were due to the fact that some of these bench-marks may have sunk slightly deeper into the ground under their own weight, thus registering too great settlements of deeper lying layers, or because other bench-marks may have been slightly pushed up as a result of swelling of the clay immediately adjoining them.

Under different conditions such bench-marks may give more consistent results. It would appear advisable to place them mainly in compact layers or relatively rigid soils, such as compact sands, and to place such underground bench-marks in several symmetrically located groups as could provide a check on each other through comparison with the performance of the foundation at the surface.

Effect of Ground-Water Variations.—Professor Hanna attempts to show that the buoyant effect of rising subsoil water is so small that it cannot account for the observed corresponding slowing down of settlements as suggested by the writer. The premises on which Professor Hanna bases this attempt are incorrect. Thus he states that when the period of construction coincides with the flood period, the corresponding increase of pressures due to the continued construction of the buildings ranges from 0.4 to 0.8 kg per sq cm. Only Buildings IV and V show any definite change in the rate of settlement during flood. An examination of Figs. 6 and 7 illustrating these buildings confirms the writer's previous statements and shows that in the three months during which the water rises the increase in their surface unit pressure was only 0.2 kg per sq cm or a

[33] "Die Tragfaehigkeit von Pfahlgruendungen," by Karl Terzaghi, *Die Bautechnik*, 1930, No. 34.

[34] "Settlement of Structures in Europe and Methods of Observations," by Karl Terzaghi, *Transactions*, Am. Soc. C. E., Vol. 103 (1938), Fig. 52(*a*), p. 1446.

value two to four times smaller than the one given by Professor Hanna. The corresponding increase in the pressures on the dark clay is only about 0.160 kg per sq cm. Thus, the value of 0.114 kg per sq cm, representing the maximum reduction of pressures on the dark clay due to buoyancy as computed by Professor Hanna, cumulatively counteracts 70% of the corresponding increase in the pressure of the building up to the moment when the peak of the subsoil-water rise is reached. This is more than sufficient to explain the observed discontinuities in the time-settlement curves of the two buildings, as has been done by the writer. Furthermore, the foregoing value of 0.114 kg per sq cm, which expresses the effect of buoyancy, is far from being excessive, as suggested by Professor Hanna, especially for Building IV. The subsoil-water rise at Building IV is much larger than the 2 m assumed by him (Fig. 5); and the coefficient 0.057 which he used (and which corresponds to 1 m water rise, of 35-lb-per-cu-ft buoyancy) is not too small since it does not take into account the capillary and entrapped moisture contained in the clay before the water rose. This should have been done for Building IV (see Fig. 5).

Professor Hanna further states that during laboratory compressibility tests which he performed "The pressures required to return a 12-mm sample to normal, when swelling occurs under the weight of the piston alone, vary between 0.18 and 0.30 kg per sq cm." The writer observed similar values during his own tests; but then Professor Hanna proceeds to attribute to these swelling pressures the observed changes in the rate of settlement of buildings and states: "Therefore, it is safe to assume that * * * the swelling of these layers reduces the pressure on the layer above them and, probably, to a slight degree, on the lower layers." The foregoing, he states, "is a mere assumption," and, the writer adds, an unwarrantable one. The pressure on the layer either immediately above or below the swelling layer will always remain equal to the superimposed weights and loads. The assumption made by Professor Hanna is equivalent to saying that an attempt to raise a load by a jack appreciably reduces both the weight of the load itself and the pressure of the jack on its support! That is (Fig. 17), if $p > 0$, then $G < W$ and $N' < N$. Obviously, this is impossible. If $p > 0$, then both $G = W$ and $N' = N$. Only "w" would decrease correspondingly—that is, the stresses in the swelling layer. This would reduce the component of the settlements due to the compression of the swelling layer itself; but, as discussed under the heading "Compressibility of Soils" herein, this settlement component is relatively small.

Conclusive evidence that the changes in the rate of settlement could not be primarily affected by swelling is provided by Building IV. Only part of that building rests on a strongly swelling brown clay, the remainder being supported by a silt which scarcely swells at all. Nevertheless, both the part supported by the brown silt (Point 16) and the part supported by the brown clay (Point 1) have shown exactly the same changes in the rate of settlement during changes in the subsoil-water level (Figs. 5 and 6). They should have shown a marked difference if Professor Hanna's assumption concerning the primary importance of swelling were correct for the cases reported in the paper.

On the other hand, the writer agrees fully with the explanation of the fact that the cracking of ancient houses in Cairo is particularly pronounced during

the yearly flood. Not only do most cases of collapses of such houses occur during the seasonal flood, but during the same period many breaks occur in the water mains of Cairo. This must be due to the light weight of such old houses and water mains and to their incapacity to resist stresses induced by unequal swelling of the soil on which they rest.

Remolding of Clays.—Professor Hanna mentions some interesting and promising recent studies he made near the sea-coast on both brown and dark clays deposited in salt water which were very unfavorably affected by remolding, whereas similar fresh-water clays were not. It is difficult to decide at present whether the obvious conclusion from these studies could be generalized, since many glacial clays, presumably deposited in fresh water, are nevertheless found to be very unfavorably affected by remolding.

Conclusions.—The studies reported in the paper have provided much data of practical importance for local foundation designs. They have further served to prove the qualitative correctness of the general theories developed by the modern science of soil mechanics, which provide a surer basis for the exercise of sound engineering judgment. At the same time they have shown the quantitative limitations attached to numerical forecasts of the behavior of undisturbed soils.

Although much of the data given in the closing discussion has served to show the errors in some of the assumptions made by the discussers, the writer is glad that these assumptions were advanced, because he believes that the exchange of views has served to emphasize the complexity of the problems connected with settlement analyses and the necessity of having numerous co-ordinated observations on different structures before conclusions may be reached even for one locality.

The writer's work in this field constantly strengthens his conviction that further progress in the knowledge concerning the actual behavior, under stress, of undisturbed soils, is not possible on the basis of fragmentary data from isolated observations. The time involved in making systematic observations, as well as other reasons, precludes the possibility of their satisfactory execution by any single person or group of individuals unless this work is assigned to them as a regular, full-time job. This consideration, as well as the large investments depending on the satisfactory and economic design of foundations, coupled with the large degree of guesswork on which present codes and design methods are based, should justify the creation of special offices in most large cities for the purpose of making control observations, of the type described, on new structures.

AMERICAN SOCIETY OF CIVIL ENGINEERS

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DISCUSSIONS

POLLUTION OF BOSTON HARBOR

Discussion

BY MESSRS. GEORGE A. SOPER, AND ARTHUR D. WESTON AND GAIL P. EDWARDS

GEORGE A. SOPER,[12] M. AM. SOC. C. E. (by letter).[12a]—Every one will be grateful to the authors of this valuable paper for putting on record the important results of the investigations of the pollution of Boston Harbor. Had the studies made of New York Harbor been presented to the Society in suitable papers at the time they were made, there is little doubt that they would have been of much greater value to the profession. As it is, the work of the New York investigators, up to 1914, must be sought in the three volumes of the Metropolitan Sewerage Commission, which were published in 1910, 1912, and 1914, and as these editions were limited, the information is not always to be found even in important libraries.

The conditions attending the discharge of sewage into Boston Harbor have been of much interest in connection with the investigations which have been made of New York Harbor.

The New York Bay Pollution Commission, which existed between 1903 and 1906, took careful account of the conditions as they were reported in the official reports and looked upon them as a warning of conditions which should never be allowed to occur in the harbor of New York.

Some years later the members of the Metropolitan Sewerage Commission of New York went to Boston and visited the Deer Island, Moon Island, and Peddocks Island outlets, to see for themselves the results of discharging sewage under the three methods used. The reasons for the works and what they were intended to accomplish had been made familiar to them through reports and correspondence. The chief engineer of the Massachusetts State Board of Health was invited to consult on the New York work, because of his familiarity with the disposal of the sewage of Boston and vicinity.

NOTE.—This paper by Arthur D. Weston and Gail P. Edwards, Members, Am. Soc. C. E., was published in March, 1939, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: June, 1939, by E. Sherman Chase, M. Am. Soc. C. E.; December, 1939, by Messrs. F. E. DeMartini, and A. M. Rawn; and February, 1940, by Harrison P. Eddy, Jr., and Samuel A. Greeley, Members, Am. Soc. C. E.

[12] Cons. Engr., New York, N. Y.

[12a] Received by the Secretary February 23, 1940.

Not being able otherwise to obtain all the information wanted, the Metropolitan Sewerage Commission sent its floating laboratory, *Quadrant*, to Boston to make studies, especially of dissolved oxygen, by the identical method which had proved successful in the Commission's hands in studying the digestive capacity of New York Harbor for sewage.

Throughout its eight years of existence, the Metropolitan Sewerage Commission recognized that it would not be necessary nor practicable to keep all sewage out of New York Harbor, but that the harbor had a large capacity for assimilating sewage, harmlessly and inoffensively, provided the principles under which the digestive capacity was utilized were thoroughly understood and skilfully employed.

Proper diffusion was indispensable to digestion and many experiments designed to "throw light" upon this subject were made in the laboratory, in large tanks courteously set aside in the New York Aquarium by the New York Zoological Society, and by means of properly equipped boats, and pontoons filled with sewage on Long Island Sound and in various parts of New York Harbor. There were parts of the harbor itself which were looked upon as affording exceptional opportunities for investigating the phenomena of diffusion and digestion, and as these had the merit of practical application, they received very careful attention. Before the investigations of the Metropolitan Sewerage Commission were concluded, the entire harbor of New York came to be looked upon in the light of a great laboratory where experiments on a huge scale were being conducted.

Arthur D. Weston,[13] and Gail P. Edwards,[14] Members, Am. Soc. C. E. (by letter).[14a]—In closing the discussion, attention is called to the remarks by Mr. Rawn, in which the question has been raised as to the effect on the bacterial counts of the length of time which elapsed between the collection of the samples and the "setting" of the samples, the results of which are shown in Fig. 9.

Laboratory records show that, of all the samples collected in 1936, 47% of the fermentation tubes were set the same day that the samples were collected, 50% were set the next day (usually in the forenoon), and only 3% of the samples were more than 24 hr old before setting. Confirmed tests were made on all positive fermentations.

The close correlation between the results of the bacterial examinations and the sanitary surveys would lead one to believe that any error from delay in setting has not affected the findings seriously, and to all practicable purposes these results are a fair indication of the coliform density of the waters in various parts of Boston Harbor. Undoubtedly, the bacteria density of tidal waters is affected by the plankton, but the laboratory of the Massachusetts State Department of Public Health has no definite information as to such relationship. Accordingly, without supporting data the effect of the plankton is speculative. Regardless of the killing, by plankton or other causes, one would have to assume

[13] Chf. Engr., Dept. of Public Health, State of Massachusetts, Boston, Mass.

[14] Prin. San. Chemist, Wards Island Sewage Treatment Plant, Dept. of Public Works, City of New York, Wards Island, N. Y.

[14a] Received by the Secretary February 16, 1940.

that, if the killing were as Mr. Rawn states, the harbor waters inside of the point of outfall on the outgoing tide would be nearly free from coliform organisms. Fig. 12 shows that most of the killing is done in the first 2 hr and that there is little effect after that period of time. An examination of Table 11 shows that in accordance with laboratory experiments conducted with sea water and 0.5% of sewage the same number of coliform organisms was present in the sample at the end of 24 hr as at the start. This number diminished to practically 0 at the end of 96 hr; but in the case of the 1% sewage with sea water, the reduction was from 500 to 100 in 24 hr and again to about 0 at the end of 96 hr. With 1.5% sewage added to sea water the reduction was from 1,000 to 100 in the first 24 hr, 50 at the end of 48 hr and 0 at the end of 96 hr. The results of tests made in 1935 by Paul J. Beard and Niel F. Meadowcroft of Stanford University, at Stanford University, Calif., show that 60% of *Escherichia Coli* survive for a period of one day and at the end of ten days more than 3% are still present in the water.[15] Undoubtedly there is a need for further research in the matter of the effect of plankton or other causes on coliform organisms in sea water.

It should be borne in mind that, in conducting any studies such as that in Boston Harbor, many inaccuracies can enter the study. For example, with the use of one boat a number of hours may elapse between the collection of some samples; and during this period of collection change of winds and variations in tidal conditions may play an important part. Furthermore, it should be stated that the results shown illustrate only the conditions found at the time the samples were collected; undoubtedly at other times objectionable conditions may be more pronounced. Taking all of these things into consideration, the writers feel, therefore, that the results illustrate, unusually well, the conditions disclosed by the sanitary survey.

Relative to the effect of partial treatment of the sewage before it is discharged into the harbor, on the bacterial content of the water of the harbor, it should have been stated that the recommendations do provide for chlorination of the settled sewage.

The various discussions by Messrs. Chase, Eddy and Greeley, DeMartini, Rawn, and Soper are greatly appreciated.

Correction for *Transactions*: In Tables 6 and 11, fourth columns, change "Reduction," in the column heading, to "Red" (refers to the number of acid-forming bacteria). See also errata in *Proceedings*, December, 1939, p. 1798.

[15] *American Journal of Public Health*, Vol. 25, No. 9.

AMERICAN SOCIETY OF CIVIL ENGINEERS

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DISCUSSIONS

SIMPLIFIED WIND-STRESS ANALYSIS OF TALL BUILDINGS

Discussion

BY OTTO GOTTSCHALK, ESQ.

OTTO GOTTSCHALK,[18] ESQ. (by letter).[18a]—The discussions by Professor Carpenter and Mr. Winick are to be highly commended. Equation (8) will serve as a guide toward the proper dimensioning of structural parts to account for the final elastic action of the structure, especially for earthquake design; and Mr. Winick's suggestion that the analysis of stiffness of a story be based on either lower or upper beams only is a further great simplification and convenience, especially for preliminary and tentative studies. Both upper and lower beams must be considered when k-values are not proportional; for instance, in Fig. 3(a), when the k-values of the beams in the top floor (see Line 2) are 3.5, 4, and 6, and in the bottom floor (see Line 5) 12, 6, and 4 in.^{3}, the differences between results from either upper or lower beams are only about 30% and may create a wrong picture of the distribution and sign of stresses. The upper and lower floor beams must also be given equal weight when the joint stiffnesses given in Line 7 of Fig. 3(a) are to be used for the calculation of deflection and the period of oscillation of the skeleton.

A report of exceptional value, concerning the Empire State Building, in New York, N. Y., has been presented by J. Charles Rathbun, M. Am. Soc. C. E.,[19] and also in the discussions of this paper such as that by Colin Skinner, Assoc. M. Am. Soc. C. E.,[20] who present fundamental observations not previously available to the profession. In the writer's opinion, no work on the subject of wind bracing can claim to be complete in the future without referring to that paper. It certainly proves the need for some simplified method of wind-stress analysis such as the one presented by the writer.

Even in the greatest of buildings the distribution of the many varieties of

NOTE.—This paper by Otto Gottschalk, Esq., was published in December, 1938, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: April, 1939, by Samuel T. Carpenter, Jun. Am. Soc. C. E.; and June, 1939, by Charles B. Winick, Assoc. M. Am. Soc. C. E.

[18] Buenos Aires, Argentine Republic.

[18a] Received by the Secretary December 11, 1939.

[19] "Wind Forces on a Tall Building," by J. Charles Rathbun, *Proceedings*, Am. Soc. C. E., September, 1938, p. 1335, and December, 1939, p. 1771.

[20] *Loc. cit.*, June, 1939, p. 1024.

material cannot be expected to be perfect at the beginning of its life. Nature must work with irresistible force to bring about that perfection after, say, ten or fifteen years, so that finally every kind of material will work as a part of the structure, in harmony with its physical properties. Thus, one more new and interesting responsibility rests upon the engineer to design and balance the resisting structure so that the building may be perfect as soon as possible after its completion. He must provide appropriate joints in the masonry work, from the first, in such a way as to assure its permanent co-operation in the maintenance of joint stiffness.

That new and interesting task requires careful design based upon thorough preliminary analysis for which the methods derived from current abstract theory are too complicated,[21] and for which rule-of-thumb methods and classifications recommended in textbooks are too far from actual conditions in the structures. The writer has reduced the analysis to simple, direct addition of existing $\frac{I}{L}$-coefficients, which makes comparative investigation and final design easy, rapid, and interesting. Incidentally, the method presented affords a better distribution and a considerable economy of steel.

Fundamental Conceptions.—As stated in the "Synopsis," the analysis presented is an application of the natural geometrical conception[3] based upon model statics.[2] Although the experimental or geometrical method of analysis simplifies, equally, all other comparatively intricate statical problems, the analysis of wind stresses is possibly one of the fields most in need of perfecting, and therefore it has been emphasized in the paper.

In principle, it should be remembered that, whereas the methods based on current abstract statics tend to anticipate complications over the entire structure, in the way of redundants, etc., the experiments and theory derived from the writer's method consider and deform only the stressed element that is to be analyzed and take into account only those of the surrounding structural members which are visibly affected by that deformation. When it seems necessary to refine the results, appropriate corrections are applied rather than recomputations that complicate the procedure fundamentally. In wind-stress analysis, moreover, the complications of "exact" methods are the less justified because an unknown part of the stresses, estimated as between one-half and nine-tenths, is being absorbed by brick work.

The theory explained in the paper may be further illustrated by reference to Fig. 6. A floor of the structure shown in Fig. 3(a) may be assumed to consist of three resisting elements or independent frames formed by the columns and beams of the three bays. First rotate each frame about Points B, C, and D (Fig. 6(a)) through an angle $\tan^{-1}\frac{\Delta}{h}$; fix each joint against rotation between the adjacent members meeting at that joint; and restore the vertical displacement of the two joints that were elevated in the first step. The resultant deformation is shown diagrammatically in Fig. 6(b).

[21] "Wind Bracing in Steel Buildings," Sixth Progress Report of Sub-Committee No. 31, Committee on Steel of the Structural Division, *Proceedings*, Am. Soc. C. E., June, 1939, p. 969.

[3] *Transactions*, Am. Soc. C. E., Vol. 103 (1938), p. 1019.

[2] *Journal*, Franklin Inst., July, 1926, and February, 1929.

The conclusions of the Committee on Wind Bracing[21] confirm the results of the writer's investigations. One of the conclusions may be quoted as a welcome, progressive step: "The wind-reaction ratio—that is, the ratio of vertical wind reaction of inner column to that of outer column—was seen to be wholly determined by the relative sizes of members and to be entirely under control by properly varying these relative sizes."[22] This conclusion is the more valuable because the Committee arrived at the same results along entirely different lines of approach. At the same time, the report emphasizes the necessity of an analysis, such as the one presented herein, which may be applied quickly and easily to frames of any number and length of bays. The

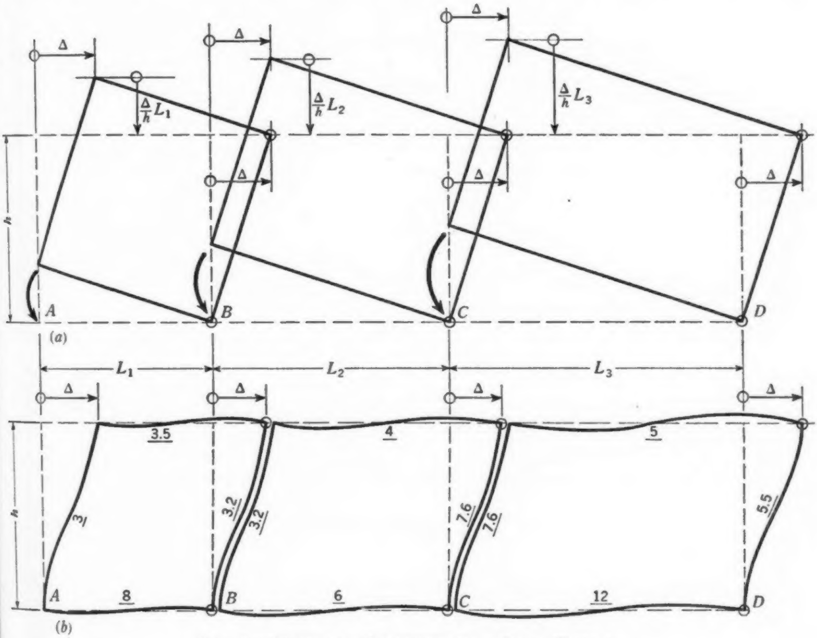

FIG. 6.—RESISTING ELEMENTS OF A RIGID FRAME

writer has applied the simplified analysis to all beams and columns of the interesting three-bay, ten-story bent of the University of Pennsylvania tests, the bending moments of which were given in the report. His analysis showed a close agreement with those obtained by the Cross method—within 0 to 2% in most cases and as much as 5% in a few exceptional cases. The same stiffness values calculated for the bending moments served to obtain the comparative deflection immediately.

Tentative Designs.—As an example of an unsymmetrical frame of six bays, consider Fig. 7. In Fig. 7(*a*) the four central spans are long in relation to the two exterior spans; in Fig. 7(*b*) they are short in the center but long at the ends; and in Fig. 7(*c*) the three left spans are long and the remaining ones are

[22] "Wind Bracing in Steel Buildings," Sixth Progress Report of Sub-Committee No. 31, Committee on Steel of the Structural Division, *Proceedings*, Am. Soc. C. E., June, 1939, p. 970.

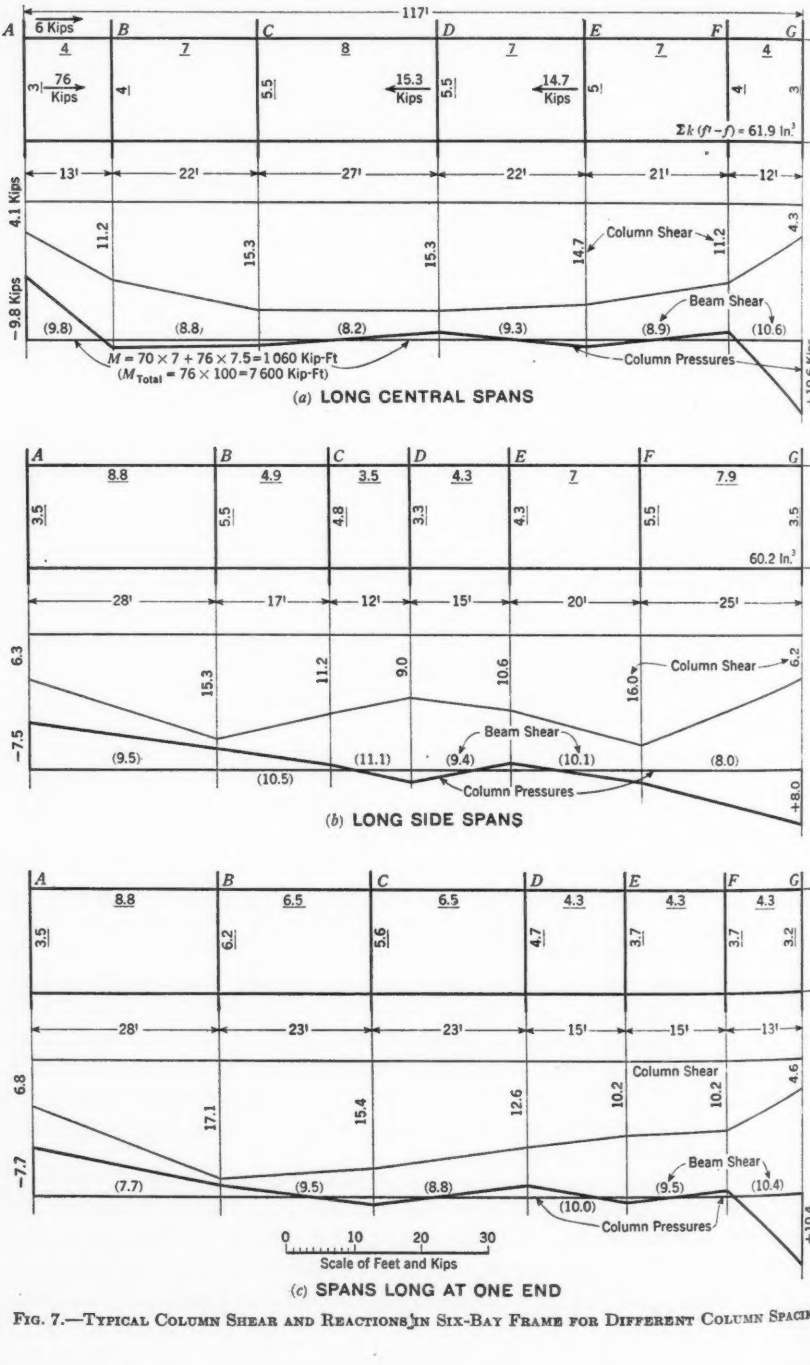

FIG. 7.—TYPICAL COLUMN SHEAR AND REACTIONS IN SIX-BAY FRAME FOR DIFFERENT COLUMN SPACINGS

short. To obtain a uniform criterion the k-values of beams in the three designs have been assumed to vary in relation to spans and the k-values of the columns in relation to half the lengths of adjoining bays. The total floor loading has been assumed at 150 lb per sq ft and the load of exterior walls at 1 kip per front ft of each story. Under each of the three arrangements in Fig. 7 there is a diagram of column shear and one of the column reactions, with the shears in the beams indicated in parentheses.

These diagrams are most instructive and their convenience can scarcely be over-estimated. Wide spacing of end spans (see Fig. 7(*b*)) make the reactions approach a planar distribution, whereas short exterior spans, such as in Fig. 7(*a*), produce the most marked differences between the reactions of exterior and the nearest interior column. This would agree with Mr. Skinner's observation:[23] "Cracks were also observed in the concrete floors at the first row of interior columns, parallel to the exterior walls." Consequently, where diagrams of reactions, such as those of Fig. 7, indicate abrupt differences between reactions at exterior spans and where prevention of cracks is considered important, joints or dummy joints are advisable at the first row of columns unless, in the architectural design, exterior spans may be widened until the reaction diagram (Fig. 7) shows a more planar distribution, although the cost of the steel frame may be higher.

Note that at reactions of the two exterior columns at each side of the structures a slight correction may be applied, as shown at the end of the paper.

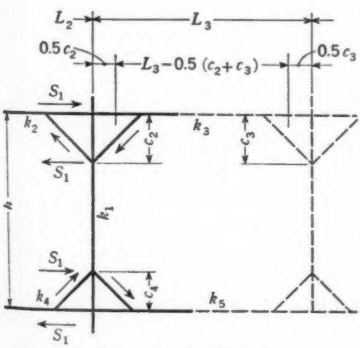

FIG. 8.—WIND BRACING

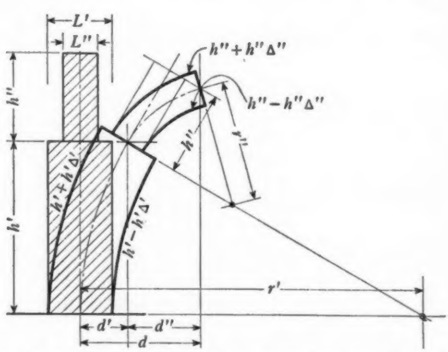

FIG. 9.—DEFLECTION OF BENT FROM UNIFORM CHANGE OF LENGTH OF EXTERIOR COLUMNS

Wind Bracing.—Knee bracing at the ends of columns has the effect of shortening the free length of both columns and beams, thus increasing the stiffness values from k to k'. For conditions shown in Fig. 8, the k'-values may be computed as follows:

$$k_1' = \frac{h\,k_1}{h - b_2 - b_4} \qquad (9)$$

in which

$$b_2 = c_2\,\frac{k_2 + k_3}{k_1 + k_2 + k_3} \qquad (10a)$$

[23] *Proceedings*, Am. Soc. C. E., June, 1939, p. 1025.

and

$$b_4 = c_4 \frac{k_4 + k_5}{k_1 + k_4 + k_5} \quad \text{.......... (10b)}$$

Furthermore,

$$k_2' = \frac{L_2 \, k_2}{L_2 - 0.5\,(c_1 + c_2)} \quad \text{.......... (11a)}$$

and

$$k_3' = \frac{L_3 \, k_3}{L_3 - 0.5\,(c_2 + c_3)} \quad \text{.......... (11b)}$$

Then, the stiffness quotients corresponding to Line 2, Fig. 3, are $\frac{k_2'}{k_1' + k_2'}$ and $\frac{k_3'}{k_1' + k_3'}$, making the shear resistance of column (Line 7, Fig. 3) $\frac{k_1' \, k_2'}{k_1' + k_2'}$ and $\frac{k_1' \, k_3'}{k_1' + k_3'}$.

For example, let $k_1 = k_2 = k_3 = k_4 = k_5$; $c_2 = c_3 = c_4 = 0.25\,h$; and $L_3 = h$. Then $b_2 = 0.25\,h \times \frac{2}{3} = \frac{h}{6} = b_3$; and

$$k_1' = \frac{k_1}{1 - \frac{1}{3}} = 1.5\,k_1 \quad \text{.......... (12a)}$$

and

$$k_3' = \frac{k_3}{1 - 0.25} = 1.33\,k_3 \quad \text{.......... (12b)}$$

and the shear resistance (Line 7, Fig. 3) equals

$$\frac{k_1' \, k_3'}{k_1' + k_3'} = \frac{1.5\,k_1 \times \frac{4}{3}}{\frac{3}{2} + \frac{4}{3}} = 0.706\,k_1 \quad \text{.......... (13)}$$

Without wind bracing, the shear resistance of that column would be:

$$\frac{k_1 \, k_3}{k_1 + k_3} = 0.5\,k_1 \quad \text{.......... (14)}$$

indicating that the increase due to knee bracing is more than 40 per cent.

Let $S_1 = \frac{k_1' \, k_2'}{k_2' + k_3'} + \frac{k_1' \, k_3'}{k_2' + k_3'}$, the shear resistance of the column (see Fig. 8); then the 45° knee bracing will receive a stress at the upper left and right sides, respectively, of $\frac{1.4\,S_1\,k_2}{k_2 + k_3}$ and $\frac{1.4\,S_1\,k_3}{k_2 + k_3}$. In steel structures the heights c_2 and c_4 of the knee bracing should be measured from the outside rivets, and in reinforced concrete structures from the most remote steel bars crossing the column.

Deflection from Wind Stresses.—A further interesting application of the simplified analysis is the calculation of deflection or sway of tall buildings by means of the shear resistance at the floors (shown at Line 7, Fig. 3), applying the actual values of $k = \frac{I}{L}$ in beams and $k = \frac{I}{h}$ in columns.

The total deflection of a structure may be analyzed as the sum of three distinct stress actions which, in the inverse order of their numerical importance, are as follows:

1. Direct Shear at Columns.—The direct shear at columns, due to wind pressure, without discounting the large portion actually absorbed by the walls, will generally not exceed 0.26 kips for every square inch of steel section of columns. For the steel, the modulus of elasticity in shear is assumed at $E_s = 13\,000$ kips per sq in. The deflection from direct shear will not exceed 0.24 in. for every 1 000 ft of height of the building and therefore, generally, may be neglected.

2. Reactions in Exterior Columns.—The pull at the left and the compression at the right exterior columns, resulting from reaction diagrams as shown in Fig. 7, lengthens the first and shortens the latter. A prism of width L' and height h' (Fig. 9), one side of which is uniformly lengthened by Δ' and the opposite side shortened by Δ' per unit of length, forms a circular arch of radius r', such that: $\dfrac{r' + \dfrac{L'}{2}}{r' - \dfrac{L'}{2}} = \dfrac{h' + h'\,\Delta'}{h' - h'\,\Delta'}$; and

$$r' = \frac{L'}{2\,\Delta'} \qquad (15)$$

At the height of h' the "geometrical" deflection d' (see Fig. 9) is $(r' - d')^2 = (r')^2 - (h')^2$; or

$$d' = \frac{(h')^2}{2\,r'} = \frac{(h')^2\,\Delta'}{L'} \qquad (16)$$

A prism of base L'' and height h'' on top of the base prism, lengthened and shortened respectively by Δ'' per unit of height, will form a curve of radius $r'' = \dfrac{L''}{2\,\Delta''}$ and deflect $\dfrac{(h'')^2\,\Delta''}{L''} + \dfrac{2\,h'\,h''\,\Delta'}{r'}$, so that the total deflection d'' on top of the upper prism is:

$$d'' = \frac{h'\,\Delta'\,(h' + 2\,h'')}{L'} + \frac{(h'')^2\,\Delta''}{L''} \qquad (17)$$

Equations (15), (16), and (17) apply also for any other difference of height, $2\,h'\,\Delta'$ and $2\,h''\,\Delta''$, caused by uniform extension or compression of both exterior columns, or in one column only with respect to the other—as, for instance, from change of temperature along one side of the building.

In the field it is not likely that exterior columns will be required to sustain an average wind stress over their total length of more than 2 kips per sq in.; assuming $E = 30\,000$ kips per sq in. (see Fig. 9), this would make $\Delta' = \Delta'' = \dfrac{2}{30\,000} = \dfrac{1}{15\,000}$. As an example, let: $h' = 400$ ft, $L' = 200$ ft, $h'' = 200$ ft, and $L'' = 100$ ft, making $h'\,\Delta' = \dfrac{4}{150}$ ft and $h''\,\Delta'' = \dfrac{2}{150}$ ft. Then, by Equation (16), $d' = \dfrac{4 \times 400}{200 \times 150} = 0.0533$ ft; by Equation (17), $d'' = \dfrac{2 \times 200}{100 \times 150}$

$+ \dfrac{2 \times 4 \times 200}{200 \times 150} = 0.0800$ ft; and (see Fig. 9) $d = d' + d'' = 0.1333$ ft $= 1.6$ in. This displacement may be termed a geometrical effect of uniform alteration at exterior columns.

3. Aggregate Sway of Floors.—Referring to Fig. 1, Equation (5), for $\Delta \lesseqgtr 1$, may be written as follows:

$$P = \frac{6\,E\,k\,(\theta\,\Delta - \theta'\,\Delta)}{h^2} \qquad (18)$$

If the deflection of any one story of height, h, being $\theta = \dfrac{f'}{h}$ and $\theta' = \dfrac{f}{h}$:

$$\Delta = \frac{P\,h^2}{6\,E\,[\Sigma k\,(\theta - \theta')]} \qquad (19)$$

For example, at the nth floor of the frame in Fig. 3, according to Line 7: $\Sigma k\,(\theta - \theta') = 31.67$ in.^{3} Furthermore: $P = 32.4$ kips; $6\,E = 180\,000$ kips per sq in.; $h = 16 \times 12 = 192$ in.; and $d = \dfrac{32.4 \times 192^2}{31.67 \times 180\,000} = 0.2095$ in. In other words, the deflection is $\dfrac{100 \times 0.2095}{16} = 1.31$ in. per 100-ft height of building. However, if the height of the floors was only 12 ft, the horizontal deflection would be only 0.72 in. per 100 ft. Although Equation (19) does not take into account the reciprocal influences of stiffness and displacements between floors, experience indicates that it presents a sound basis for calculating deflections. After more observations on existing buildings become available, it may possibly be completely corroborated.

In Fig. 10 an analysis of this part of the deflection of a structure has been illustrated. One bent only has been shown as an example. In practical application, however, all bents joined by rigid floors must be assumed to resist, jointly, the total wind pressure upon the building, taking into account possible effects of eccentricity of the wind force. Walls anchored within a bent will be treated separately.

Walls in Skeleton.—Walls tied to columns within a bent increase the rigidity and reduce the deflection of that bent. The simplified wind-stress analysis may be applied to analyze and judge the possibilities and limitations of that ever-present structural element.

A 25-story bent with a distribution of bays similar to Fig. 7(b) but with a rigid wall joining Columns B and C, as would be required for an elevator shaft or staircase, has been illustrated in Fig. 10. It is assumed that cross braces or some other form of reinforcement tie the wall firmly to Columns B and C (at least in the lower floors) so that these columns act as chords of a vertical cantilever beam fixed at the ground floor. At the same time Columns B and C act, at all the floors, as members of the frame, the same as all other columns, and they contribute to the joint wind force a resistance, $\Sigma k\,(\theta - \theta')$, at the several floors, as analyzed in Line 7 of Fig. 3.

This joint resistance may be called the "section modulus" of the bent or frame at the several floors, in the same sense as $\dfrac{I}{c}$ is defined as the section

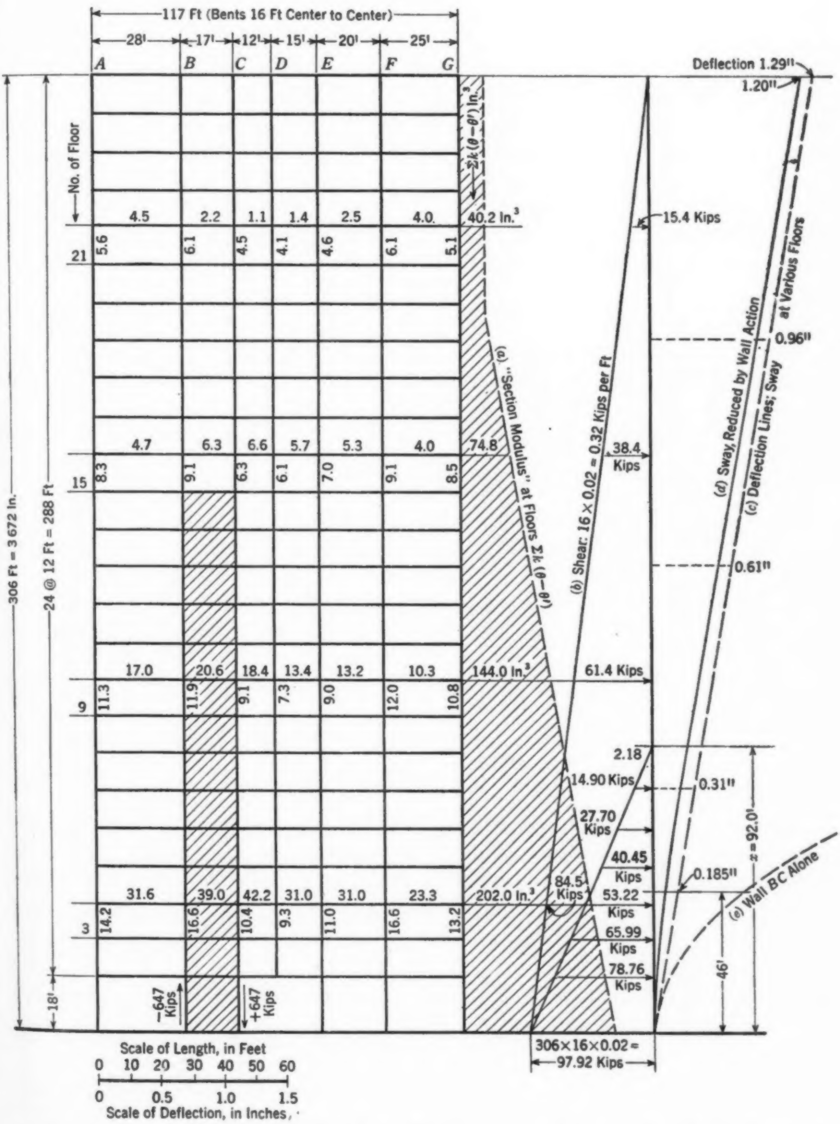

FIG. 10.—DIAGRAMS OF "SECTION MODULUS" AND DEFLECTION FOR A TWENTY-FIVE STORY BENT WITH A "WALL" BAY

modulus of an ordinary beam; and, like $\frac{I}{c}$, its dimension is in inches³. In Fig. 10 the wind pressure has been assumed to be 20 lb per sq in., distributed over the entire height of the bent so that the aggregate shear increases uniformly from 0 at the top to $w = 97.92$ kips on the bottom, as shown by the solid line at the right. Similarly, the section modulus converges from a maximum at the bottom of the structure to a minimum at one of the upper floors where only vertical loads are considered. This is shown as a dashed line in the shaded area to the right of the frame in Fig. 10. It is a valuable guide and check for rational proportioning at intermediate floors. At the upper part of that diagram it is interesting to note that, beams being equal in the several floors, the "section modulus" increases only about 20%, which represents the increase of $k = \frac{I}{h}$ in the columns.

Similarly, the deflection derived from total sway at floors of equal height increases uniformly from the bottom to the top as indicated by Curve (*c*), Fig. 10, and need be calculated at a few floors and at the top only. From the second to twenty-fifth floor (Fig. 10) the story height is uniformly 12 ft so that Equation (19) becomes $\frac{h^2}{6\,E} = \frac{144^2}{6 \times 30\,000} = 0.1152$; and $\Delta = \frac{0.1152\,P}{\Sigma k\,(\theta - \theta')}$.

Curve (*c*), Fig. 10, has been drawn, assuming that the frame supports all of the wind load, without considering the resistance and the wall *B C*. In the bottom floors, Columns *B* and *C* (which form the chords of the cantilevered wall) have areas of 69 and 45 in.² and moments of inertia of 5 100 and 2 100 in.⁴, respectively; and they are 17 ft (or 204 in.) apart. The moment of inertia of the wall, therefore, is: $I = 5\,100 + 2\,100 + \frac{69 \times 45 \times 204^2}{69 + 45} = 114.06 \times 10^4$ in.⁴ and $E\,I = 342.18 \times 10^8$ kip-in.²

The wall alone, as a free cantilever 306 ft (or 3 672 in.) long, loaded uniformly by $16 \times \frac{0.02}{12} = 0.0267$ kip per in. deflects as shown by Curve (*e*), Fig. 10, and would neither contribute to stiffness nor reduce deflection. However, since Columns *B* and *C* are part of the frame, one may assume that the wall *B C* acts as a cantilever fixed at the bottom and loaded at the several floors. The shear decreases uniformly from 97.92 kips at the bottom to 0 at some level, distant x above the ground, so that at $\frac{x}{2}$ the deflection of the cantilever would be equal to the sway of the skeleton without the wall *B C*.

TABLE 5.—ANALYSIS TO DETERMINE THE DEFLECTION OF A TWENTY-FIVE-STORY BENT (SEE FIGURE 10)

Floor	External load, P, in kips	$\Sigma k(\theta - \theta')$, in inches³	Deflection Δ, in inches per 12 ft of height	Number of stories included in section	Deflection Δ, in inches per section	Total deflection, in inches
(1)	(2)	(3)	(4)	(5)	(6)	(7)
3	81.5	202.0	0.0482	6.5	0.312	0.312
9	61.4	144.0	0.0491	6.0	0.295	0.607
15	38.4	74.8	0.0592	6.0	0.355	0.962
21	15.4	40.2	0.0442	7.5	0.332	1.294

According to Table 5 the sway line of the lowest section starts from 0 at the bottom and increases at the rate of $\frac{0.0482}{144} = 3.347 \times 10^{-4}$ in. per in.

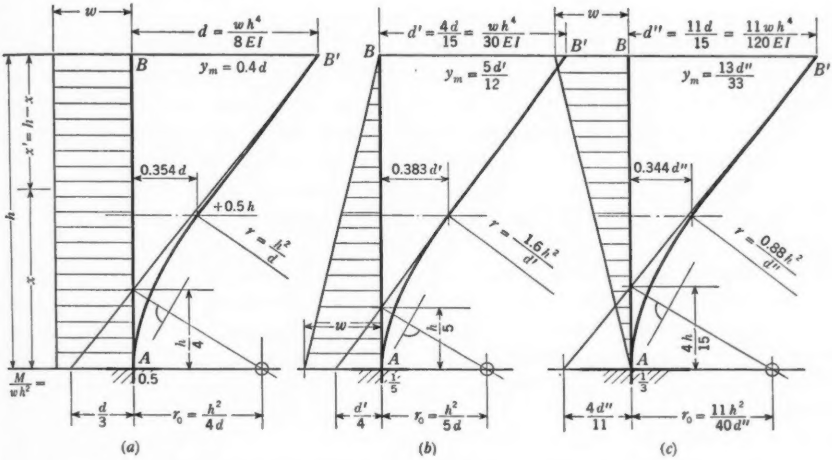

FIG. 11.—DEFLECTION CURVES OF CANTILEVER BEAMS FOR (*a*) UNIFORM LOAD, AND (*b*) AND (*c*) TRIANGULAR LOADS

Substituting, for the pressures at the several floors, a distributed load which varies uniformly from $\frac{97.92}{144} = 0.68$ kip per in. at the bottom to 0 at a distance x, the value of x (Fig. 10) can be obtained from the following equation (see Fig. 11(*b*)):

$$0.383 \times 0.68 \times \frac{x^4}{30\,E\,I} = 3.347 \times 10^{-4}\,x \ldots\ldots\ldots\ldots (20)$$

Therefore, $x^3 = 1.31924 \times 10^9 = 1\,096.8$ in. $= 91.4$ ft—that is, approximately 92 ft. The deflection at $\frac{x}{2}$ is $d = 46.0 \times 12 \times 3.347 \times 10^{-4} = 0.1848$ in. and therefore the reduced deflection of the bent, due to the wall, is $0.5\,d = 0.0924$ in.

The bending moment of the wall, at the bottom (see Fig. 10), is $M_0 = 2.13 \times 90 + 14.9 \times 78 + 27.7 \times 66 + 40.45 \times 54 + 53.22 \times 42 + 65.99 \times 30 + 78.76 \times 18 = 10\,999.00$ kip-ft, and therefore the additional reaction at Columns B and C to be resisted is $\pm\,\frac{10\,999.00}{17} = \pm\,647$ kips.

On the other hand, the section modulus at the floors below x may remain constant, thus saving the additional steel in beams which the increase of "section modulus," according to the shaded area in Fig. 10, would require without the wall action.

Cantilever walls may start at the ground floor or at any upper floor at which fixation is assured by three horizontally rigid floors capable of transmitting the fixing stresses to other rigid walls below. This facilitates the design

of offsets in walls without lessening the wind stiffness of the structure and may also be of temporary value to make up for local lack of rigidity, especially during construction and before the exterior walls have been placed in all of the floors.

To produce the "wall effect" the "wall" need not be resistant and rigid in itself. Cross braces, reinforced continuous plaster, etc., may also be designed to produce the "wall" action. A rigid panel within each of the flexible stories, one on top of the other, is certain to absorb a disproportionate part of the wind shear of every floor and, as shown by Figs. 3 and 6, to throw excessive reactions to Columns B and C, producing deformations and cracks as stresses accumulate, especially toward the lower floors of the building. This action has been confirmed by competent observation:[24] "There were relatively few cracks in the plastered ceilings; but the plastered partitions surrounding the elevator shafts were seriously cracked in the lower portion of the main tower and cracked to a lesser degree in the upper stories."

Inconveniences of that type are quite common around shafts or staircases of buildings of even moderate height, and prove the necessity for studying, in every case, the merits of utilizing or discontinuing the wall effect. The example illustrated in Fig. 10 shows that phenomenon and demonstrates how excessive sway at the top of a tall building may be reduced by reinforcement at the bottom, although at considerable expense.

Bending Lines of Cantilevers.—For possible combinations of cantilever walls the following data referring to a beam A B of constant section, fixed at Point A (Fig. 11), will be of interest. When the moment of inertia I is decreasing uniformly from the bottom (A) to the top (B), it may be taken as the I-value at about $x = \frac{h}{4}$ above the bottom. A fairly close approximation is obtained by substituting tangents to the curves at Points A and B'. The last three lines of Table 6 are based on that assumption, with $y = 0$ for the lower heights, $\frac{h}{4}$, $\frac{h}{5}$, and $\frac{4\,h}{15}$, respectively.

Earthquake Stresses.—In territories subject to earthquake shocks, it now seems customary to specify, for the statical computation, a horizontal force at each floor equal to one-tenth of the overlying vertical load. This will make the horizontal force in normal buildings six to twelve times the usual wind pressure, and the additional shears and reactions will become really important even for low buildings; and the simplified analysis will be so much more necessary. The following definition by Mr. Jacob J. Creskoff[25] may be quoted:

> "The seismic force, $F = W\,K$, acts against the mass of a building. Assuming that the mass is concentrated at the floor levels, and that floors are horizontal girders that deflect as units, then the columns and beams between two consecutive floors may be regarded as vertical beams—deflecting equally because of a concentrated horizontal force acting against the upper floor—and with end fixities determined by the degree of fixity between the vertical beams and the floors. Under these assumptions, the seismic force is distributed to all of the vertical beams, and to each in proportion to its relative rigidity."

[24] *Proceedings*, Am. Soc. C. E., June, 1939, p. 1026.

[25] "Earthquakes and Structures," by the late Leander M. Hoskins, Esq., and John D. Galloway, M. Am. Soc. C. E.; discussion by Jacob J. Creskoff, *loc. cit.*, April, 1939, p. 725.

To take care of these elementary forces, the simplified wind-stress analysis will apply directly, especially Equation (8); beyond that it will facilitate the distribution of columns along the lines suggested in Fig. 7 and contribute to the formation of a resisting nucleus within the structure to cope best with the effects of oscillation.[26]

TABLE 6.—DEFLECTION CURVES FOR A CANTILEVER BEAM, *A B*, FIXED AT A POINT, *A*, OF CONSTANT SECTION

Description (see Fig. 11)	DISTRIBUTION OF LOAD		
	Uniformly distributed w	Decreasing from w at the bottom to zero at the top	Decreasing from w at the top to zero at the bottom
Moment, M_0.......	$\frac{w\,h^2}{2}$	$\frac{w\,h^2}{6}$	$\frac{w\,h^2}{3}$
Deflection at Point B	$d = \frac{w\,h^4}{8\,E\,I}$	$d' = \frac{w\,h^4}{30\,E\,I}$	$d'' = \frac{11\,w\,h^4}{120\,E\,I}$
Average deflection, y_m	$\frac{2\,d}{5}$	$\frac{5\,d'}{12}$	$\frac{13\,d''}{33}$
Deflection, y........	$\frac{x^2\,d}{3\,h^2}\left(6 - \frac{4\,x}{h} + \frac{x^2}{h^2}\right)$	$\frac{5\,x^2\,d'}{4\,h^2}\left(\frac{2\,x'}{h} + \frac{x^2}{h^2} - \frac{x^3}{5\,h^3}\right)$	$\frac{5\,x^2\,d''}{11\,h^2}\left(4 - \frac{2\,x}{h} + \frac{x^3}{5\,h^3}\right)$
Tangent, $\frac{dy}{dx}$........	$\frac{4\,x\,d}{h^2}\left(\frac{x'}{h} + \frac{x^2}{3\,h^2}\right)$	$\frac{5\,x^2\,d'}{4\,h^3}\left(\frac{4\,h}{x} - 6 + \frac{4\,x}{h} - \frac{x^2}{h^2}\right)$	$\frac{5\,x\,d''}{11\,h^2}\left(8 - \frac{6\,x}{h} + \frac{x^2}{h^2}\right)$
Curve $= \frac{1}{r} = \frac{d^2y}{dx^2}$...	$\frac{4\,d}{h^2}\left(1 - \frac{2\,x}{h} + \frac{x^2}{h^2}\right)$ $\left(\text{in which } h^2 = \frac{4\,(x')^2\,d}{h^4}\right)$	$\frac{5\,x\,d'}{16\,h^3}\left(\frac{h}{x} - \frac{3\,x'}{h} - \frac{x^2}{h^2}\right)$	$\frac{20\,d''}{11\,h^2}\left(2 - \frac{3\,x}{h} + \frac{x^3}{h^3}\right)$
	$x > \frac{h}{4}$	$x > \frac{h}{5}$	$x > \frac{4\,h}{15}$
Approximate, y.....	$\frac{(4\,x - h)\,d}{3\,h}$	$\frac{(5\,x - h)\,d'}{4\,h}$	$\frac{(15\,x - 4\,h)\,d''}{11\,h}$
Approximate, $\frac{dy}{dx}$....	$\frac{4\,d}{3\,h}$	$\frac{5\,d}{4\,h}$	$\frac{15\,d}{11\,h}$

Stresses in Existing Buildings.—The simplified wind-stress analysis may be used to advantage in determining stresses of skeletons in existing buildings, the investigation of which should be stimulated to arrive at a better knowledge of the permanent stiffness conditions of structures and of the portion of stresses definitely absorbed by masonry work.[27]

Unless perfect measuring and recording instruments are available in abundance, it is convenient,[27] and economical, to place small, flat, metal boxes in the base layer of existing floors, next to the columns, so that they follow safely the vertical oscillations of the latter. The boxes should be sealed hermetically and should contain an easy flowing liquid. Two boxes communicate by means of small rubber or metal tubes placed out of the way along

[26] Proceedings, Am. Soc. C. E., December, 1938, p. 1993.

[27] "Wind Forces on a Tall Building," by J. Charles Rathbun; discussion by Otto Gottschalk, *loc. cit.*, May, 1939, p. 880.

partitions, etc. The slightest lifting of one of the boxes will cause at the communicating box an overflow into an attached receptacle, or glass tube, which may be weighed, measured, or read annually or semi-annually; the sum of the overflow of each pair of boxes indicates the maximum change in level between the two respective columns. If the boxes are $2\frac{1}{2}$ in. by 4 in. by 8 in., every 0.01 in. change of level shows a column of liquid 6.52 in. long in a $\frac{1}{4}$-in. glass tube, so that even $\frac{1}{1\,000}$ in. can be estimated.

It will be advisable, if possible, to set a series of pairs of boxes at two or three different floors in tall buildings; and, at one or two floors below or above each series, one pair should be set at each of the exterior panels (see Fig. 12). Assuming a story height of 12 ft, a change of level of 0.01 in. between two floors represents an additional column pressure of 2 kips per sq in. of column section.

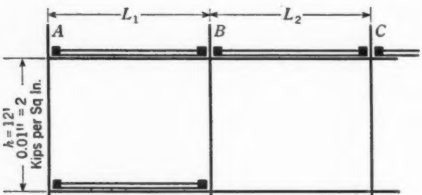

FIG. 12.—ARRANGEMENT OF BOXES FOR RECORDING RECIPROCAL CHANGES OF LEVEL AT COLUMNS

For example, consider the floor in Fig. 7(*c*) to be subjected to a total wind moment of 7 600 kip-ft. The first step is to analyze the frame according to Fig. 3, obtaining the diagram of reactions shown in Fig. 7(*c*). This supplies the sign of column pressures and, after some practice, permits the designer to estimate the reactions of most of the interior columns, thus simplifying readings at the building. Suppose the readings are as follows, in kips: $AB = -42.2$; $BC = -17.9$; $CD = +13.2$; $DE = -12.3$; $EF = +10.7$; and $FG = -81.6$. The seven unknown column reactions A, B, $\cdots$ G are calculated as follows:

	Kips		Kips
$A - B$	$= -\ 42.2$	$B = 42.2 - 55.3 =$	$-\ 13.1$
$A - C = -42.2 - 17.9$	$= -\ 60.1$	$C = 60.1 - 55.3 =$	$+\ 4.8$
$A - D = -60.1 + 13.2$	$= -\ 46.9$	$D = 46.9 - 55.3 =$	$-\ 8.4$
$A - E = -46.9 - 12.3$	$= -\ 59.2$	$E = 59.2 - 55.3 =$	$+\ 3.9$
$A - F = -59.2 + 10.7$	$= -\ 48.5$	$F = 48.5 - 55.3 =$	$-\ 6.8$
$A - G = -48.5 - 81.6$	$= -\ 130.1$	$G = 130.1 - 55.3 =$	$+\ 74.8$
$A+B+C+D+E+F+G$	$=\ 0.0$	$A = -387.0 \div 7 =$	$-\ 53.3$
$7A$	$= -\ 387.0$	$A+B+C+D+E+F+G =$	0.0

Inverting the process illustrated in Fig. 3(*a*), one may obtain from those column reactions all other stresses of columns and beams of the structure which are desired. The differences between reactions analyzed and recorded will indicate the proportion absorbed by masonry work.

In conclusion the writer hopes that he has shown an efficient method of analyzing wind stresses in tall buildings. Since the inherited abstract conception of structural statics has failed to provide engineers with adequate methods

of analysis in this field of design as well as in others, it must be replaced by more natural devices and lines of reasoning. Apparently, skyscrapers are neither uneconomical nor harmful to street traffic,[28] and the engineer can contribute much to encourage that most interesting field of technical activity. By wise distribution of materials the proper analysis of the steel frame will insure against annoying vibrations and unsightly deteriorations, and at the same time reduce the cost.

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[28] Editorially, citing Henry Wright before the American Institute of Steel Construction, *Engineering News-Record*, October 26, 1939, p. 40 (Vol. p. 526).

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DISCUSSIONS

DESIGN OF A HIGH-HEAD SIPHON SPILLWAY

Discussion

BY ELMER ROCK, JUN. AM. SOC. C. E.

ELMER ROCK,[18] JUN. AM. SOC. C. E. (by letter).[18a]—The discussions are of a high caliber and were written by engineers well qualified to criticize, constructively, siphon design procedure. Unquestionably the value of the paper has been greatly enhanced by these contributions.

Mr. Stevens has selected Siphons Nos. 5, 6, and 7 at Leaburg, and the two siphons at Walterville for the purpose of comparing prototype measurements with predicted velocities based on assuming free vortex flow in the upper bend. He emphasizes the fact that in these particular siphons the barrel is occupied only in part by water moving downstream and in part by eddies. It is doubtful that the designer of these siphons intended to have a portion of the tube non-effective. This phenomenon is not characteristic of all siphons. At Leaburg the total head is approximately 27 ft and one would not expect separation of the jet from the siphon tube. However, upon investigation it is found that the cross-sectional area of the tube at the outlet is 1.5 times the throat area. The jet issuing from the throat section is unable to follow the expanding barrel, under those conditions, with the result that part of the area is rendered non-effective, velocities are increased, and extremely low absolute pressures are developed. At Walterville the total head is approximately 46 ft and the lower leg has a constant area throughout its length. In this case separation of the jet from the barrel of the siphon could be prevented by tapering the lower leg so that the outlet area is reduced. In the opinion of the writer a siphon, in order to operate smoothly and without vibration, should be designed to eliminate parting of the water from the siphon tube, cavitation, and the presence of eddies or turbulent water pockets.

When the water leaves the invert of the upper bend, as it does at Walterville and Leaburg, a new boundary is formed and its radius is no longer equal to the radius of the invert, but somewhat greater. It is difficult to determine

NOTE.—This paper by Elmer Rock, Jun. Am. Soc. C. E., was published in April, 1939, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: June, 1939, by J. C. Stevens, M. Am. Soc. C. E.; October, 1939, by Messrs. B. E. Torpen, G. E. Hyde, and R. B. Cochrane; and November, 1939, by I. M. Nelidov, Assoc. M. Am. Soc. C. E.

[18] Asst. Civ. Engr., U. S. Engr. Dept., U. S. Engr. Office, Providence, R. I.

[18a] Received by the Secretary February 19, 1940.

the exact value of this increased radius. Accordingly, the values of r_2 that Mr. Stevens used in Table 2 are not correct and the values for radial acceleration at the crest are misleading. It is obvious that if r_2 is increased the radial acceleration ($V_2\ r_2$) will increase, resulting in better agreement with the radial acceleration at the crown. Aside from this there are other variations that should be considered. For instance, it may not be justifiable to place full trust in prototype measurements where near absolute zero pressures exist and flow conditions are extremely disturbed. In his tests[19] Mr. Stevens has found that a piezometer connection registering very low absolute pressure would suddenly spout water up in to the mercury gage. This indicates that conditions are highly unstable in the upper bend of a siphon in which the jet is allowed to separate from the walls, and measurements made under such circumstances might not be entirely reliable. From the foregoing it follows that Mr. Stevens has not shown conclusively that the assumption of free vortex flow in the upper bend of a siphon is invalid.

Flow conditions in a pipe bend (such as the upper bend of a siphon) are very complex and are not susceptible of exact mathematical analysis. The problem is made even more complex when the velocity distribution in the conduit, as it approaches the bend, is unbalanced. The shape and alinement of the conduit immediately down stream from the bend likewise influence the flow around the bend. Mr. Stevens states: "The necessity of admitting water to the summit section through a priming conduit is one factor that may nullify the application of the vortex principle." Not all siphons have priming conduits which permit water to enter in this manner. Naturally, in those that do have such provision, there is a tendency further to complicate the velocity distribution in the upper bend. According to the analysis made by Messrs. Torpen, Hyde, and Cochrane, shown graphically in Fig. 4, the effect of such admission of water in the Leaburg siphons does not influence the velocity distribution enough to nullify the application of the vortex principle.

The writer disagrees with Mr. Stevens in the contention that a mean velocity of 40 ft per sec or more would be found in the lower leg if the siphon used for illustration were in use and under observation. Perhaps this would be true if the outlet were not reduced in area. To omit the reducer would result in some increase in discharge which in turn would be accompanied by pressures close to absolute zero and separation of the water from the walls of the siphon barrel. In this case a contracted effective area might well give mean velocities as high as 40 ft per sec. The siphon designed in the problem selected for illustration, however, will discharge no more than 350 cu ft per sec, and it is the writer's contention that such a discharge (in an upper bend of the dimensions given) will not produce the phenomenon discovered at the Walterville and Leaburg siphons.

It has been reported that the Walterville siphons vibrate to a certain extent at full flows.[20] It is reasonable to attribute such vibration to the unstable condition of flow mentioned in the foregoing paragraph.

It is true, as Mr. Stevens states, that open channels are cheaper than siphon barrels. There are many instances, however, in which the flow must be carried

[19] "On the Behavior of Siphons," by J. C. Stevens, *Transactions*, Am. Soc. C. E., Vol. 99 (1934), p. 998.
[20] *Loc. cit.*, Vol. 104 (1939), p. 1805.

to the point of discharge in a closed conduit. For example, consider the case of a dual-purpose reservoir built primarily for flood control, but which at the same time is to create a lake for recreational purposes. The dam is an earth-fill embankment with the conventional flood outlet consisting of a tunnel with a gate control-tower located approximately at its mid-point. A siphon spillway, to regulate the level of the recreation pool, is to be incorporated in the tower. It draws water from the emergency gate wells and discharges into the tunnel down stream of the service gates. For a project of this description there is no alternative; the flow must be carried in a closed conduit.

When the writer first used the free vortex theory in analyzing flow conditions in the upper bend of a siphon, he too was skeptical as to its usefulness. After making several comparisons with actual test data there remained no question as to its value. The reaction on Messrs. Torpen, Hyde, and Cochrane apparently was similar. It is undoubtedly true that the paper would have been more convincing if comparison with actual measurements had been presented. It should be remembered, however, that the upper bend of a siphon is only a bend in a closed conduit, and that free vortex flow has been shown to approximate, closely, the actual velocity distribution in bends by others. Nevertheless the comparison given by the authors will be of value to those who doubt the application of the vortex theory. Once it has been shown that the velocity distribution at the summit section is far from uniform (and this is now definitely established) it follows that a plot of a computed hydraulic gradient based on mean velocities will not reveal the true diminution of absolute pressure intensities in the upper bend.

Mr. Nelidov disagrees with the application of the term "high-head siphon." He proposes to apply this term only to those siphons in which the operating head exceeds the "limiting head." As defined, the limiting head is that which develops the maximum discharge capacity of the siphon. In order to compute it the dimensions of the siphon to be classified must be known. In other words, a siphon would have to be designed first and then classified. According to this reasoning any siphon could be either put into, or taken out of, the high-head class by making some small dimensional changes. For instance, the siphon used for illustration by the writer would cease to be a high-head siphon if the outlet area were reduced slightly. This makes Mr. Nelidov's definition seem irrational. The classification according to head as interpreted by the writer is by no means new. Perhaps to restate the definition in a little different manner will serve to clarify its meaning—a siphon naturally falls into the high-head class where the operating head is so great that, to get good performance (and this means that a break in the water column or its separation from the conduit walls is prevented), special provision must be made to consume excess head. Throttling the outlet is a convenient way to expend excess head.

The diagrammatic sketch in Fig. 5 is very comprehensive and will aid the designer in visualizing flow conditions in the upper bend. The writer has followed through Mr. Nelidov's derivation and finds that it is mathematically correct. It is important to note that the entire procedure is predicated on the same assumptions relative to flow in the upper bend as made by the writer; namely, that velocity distribution is according to the free vortex theory. Except for a slight rearrangement of terms, Equation (17) is the same as Equation

(10). Where the variation in velocity in curvilinear flow can be expressed as an equation it is possible to determine, mathematically, the average kinetic energy per unit of flow. In this manner Equation (16) is useful for the case at hand. Mr. Nelidov illustrates how the priming head and the head losses from the inlet to the throat can be included in the computations. There is some question as to whether it is justifiable to include such small forces when the basic assumptions regarding flow are only approximate, and the head losses involved are difficult to evaluate. Actually the elevation of the forebay and the head lost in the siphon are directly related to the pressure at the summit section. It may not be commensurate, however, with the general accuracy of the method to compute the effect of priming head and head losses in the upper leg which, incidentally, act to offset one another. In the problem used for illustration the only effect that neglect of these factors has is to increase the negative pressure at the throat slightly. Since the permissible negative pressure of 24 ft of water has a factor of safety in it, this is inconsequential. The capacity remains unchanged.

The value K_1' from Table 1 is the sum of the losses between Section 1 and Section 2, and should be 0.186 instead of the 0.154 used by Mr. Nelidov. In checking through the arithmetic the writer finds that Equation (21) yields a value for H_{lim} of 73.9 instead of 78.5. The substitution in Equation (22) as shown is incomplete, but the answer is correct.

Mr. Nelidov calls attention to the limitations of Equation (4). Naturally when r_c becomes zero (that is, the crest is a knife-edge) Equation (4) gives a discharge of zero. This is true because, theoretically, water moving at any finite velocity, no matter how low, cannot change its direction without some curvature, however small the radius may be. In the opinion of the writer, except where hydraulic performance is of no consequence, the upper bend of a siphon should be formed with regular curves of moderate radii so that the formation of cavities is prevented. It is now a matter of common knowledge that the discharge increases when a cavity (space filled with eddying water) of this nature is filled with masonry, in spite of the fact that the flow cross-section is contracted. Equation (4) will be valid under these conditions. At the same time the writer recognizes that siphons have been built, and will be built, with exceedingly sharp crests, and that in checking the performance of these siphons the minimum value of r_c, as mentioned by the writer, is of paramount importance.

The question of selecting upper bend radii and throat dimensions arises. Many considerations enter into this problem. Intentionally avoiding these details, because it was feared they might detract from the original purpose of the paper, the writer selected radii which, in his opinion, gave satisfactory proportions. Mr. Nelidov points out that if a value for r_s of 7.0 is used Equation (4) reads $Q_1 = 39.3\, r_c \log_e \frac{7.0}{r_c}$, and that if the first derivative of Q_1 with respect to r_c is set equal to zero, a value of r_c can be determined which will give a maximum value for Q_1. For the case at hand, this procedure produces throat dimensions of $d = 4.43$ ft and $B = 3.40$, giving a throat area of 15.06 sq ft. Comparison with the throat dimensions of the siphon illustrated in Fig. 2 indicates that the proportions selected by the writer are superior on three counts:

(1) The throat area is considerably smaller, thus decreasing the cost; (2) the priming qualities are better since the length of the crest is greater; and (3) the transition from the rectangular throat to the circular lower leg is easier. The variation of throat dimensions with the upper bend radii can best be represented graphically. In this connection it can be shown that Equation (4), having a given value of r_s inserted, and plotted as a curve (r_s against A_t, in which A_t is equal to $Q_{\text{design}} \times$ throat depth $\div Q_1$) for a definite siphon capacity, will give smaller throat areas for greater values of r_c, but the throat width (or crest length) will increase rapidly. Obviously there is a practical limit depending upon such considerations as priming qualities, transition in section, and structural requirements.

Perhaps it is well that Mr. Nelidov holds open for discussion the matter of maximum permissible negative pressure. A vacuum head of 24 ft was recommended because it is a reasonable value and has been used in the past with success. It was not the intention of the writer to suggest that hydraulic engineers use the value blindly and without regard for special considerations—for example, operation at extremely high altitudes, or where the quantity of air dissolved in the water is excessive. Each case should be analyzed individually by the designer. If all influencing factors were susceptible to exact analysis it would be perfectly proper to use a vacuum head closer to the maximum of 34 ft. Owing to unforeseen disturbances and unpredictable behavior, the value used should contain a factor of safety. It is common practice in structural design to use a working stress for steel of 16 000 or 18 000 lb per sq in. Actually steel will withstand stresses up to the elastic limit of 32 000 lb per sq in. before it fails. One reason for using the lower stress is to allow for unpredictable secondary stresses.

One method of procedure sometimes resorted to for determining velocity distribution, that has not been discussed, but which warrants mention, is the utilization of the flow net or streamlines. For example, it may be necessary to find the distribution of velocity in the upper leg of a siphon. This can be done by drawing out the streamlines to a large scale.

It is often true in a paper of this nature that there is a tendency on the part of those participating in the discussion to single out certain points and so concentrate their criticism that the broad aspects of the problem as a whole are lost from sight. The writer's aim was to answer certain questions arising in the mind of the engineer confronted with the task of designing a high-head siphon spillway. That atmospheric pressure is a governing force in simple siphonic action is not difficult to understand. On the other hand, the design of a siphon spillway where the operating head is much greater than 34 ft requires a rigorous analysis to be able to guarantee, within practical limits, a fixed capacity and smooth hydraulic performance without vibration and pulsation.

In concluding this discussion the writer wishes to emphasize two points: (1) The assumption of free vortex flow in the upper bend is a valuable aid in designing a siphon; and (2) there is no limit to the operating head for which a siphon can be designed. If the head is greater than 34 ft the only special requirement is that the siphon conduit be formed in such a manner that a break in the water column or its separation from the conduit wall is impossible.

AMERICAN SOCIETY OF CIVIL ENGINEERS

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DISCUSSIONS

FLASH-BOARD PINS

Discussion

By Harry H. Hatch, M. Am. Soc. C. E.

Harry H. Hatch,[14] M. Am. Soc. C. E. (by letter).[14a]—In practice, the authors' simplified horizontal moment formula is used in the design of flash-board pins. The equation is readily obtained from Fig. 10(*e*), in which $a_1 = \frac{h}{2}$ and $a_2 = \frac{h}{3}$, and it is usually given in the form:

$$M = \tfrac{1}{2}\, w\, h^2\, L \left(H_1 + \frac{h}{3} \right) \qquad (25)$$

Expressed in inch-pounds, and with $w = 62.5$, Equation (25) becomes Equation (19*b*).

The reduction in the value of the modulus of rupture per unit area, with the increase of the pipe diameter, as demonstrated by the authors, is interesting and of vital importance. The authors report a 20% reduction in the unit rupture value between pipes of ¾-in. and 3-in. diameters. This raises the question regarding pipe sizes not within the range of the authors' tests. Will the same rate of reduction in unit value apply to larger diameters? In the absence of laboratory test results with which to answer the question, data from actual structures will be needed. For this reason, the following data may prove useful.

During the flood of September 21, 1938 (with a run-off of 600 cu ft per sec per sq mile), a section of the Cobble Mountain spillway flash-board, 44.92 ft long, functioned properly, in complete accord with its design. The pins consisted of 5-in., standard, wrought-iron pipes, and in this section they were spaced at 4.083-ft centers. The height of the flash-board was 7 ft and $H_1 = 2.25$ ft. Then: From Equation (19*b*), $M = 343\,840$ lb-in.; from standard pipe tables, $D_e = 5.563$ in. and $D_i = 5.045$ in.; from Equation (1), $S = 5.47$; and from Equation (2), $f = 62.9$ kips per sq in.

Note.—This paper by Chilton A. Wright and Clifford A. Betts, Members, Am. Soc. C. E., was published in May, 1939, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: November, 1939, by Messrs. William P. Creager, Lincoln W. Ryder, and E. T. Schuleen; December, 1939, by John E. Field, M. Am. Soc. C. E.; and January, 1940, by Julian H. White, Assoc. M. Am. Soc. C. E.

[14] Div. Engr., Springfield Water Works, Springfield, Mass.

[14a] Received by the Secretary January 23, 1940.

The average of the several measurements on some of the pipes resulted in $D_e = 5.60$ in. and $D_i = 5.07$ in., with the corresponding $S = 5.659$ and $f = 60.8$ kips per sq in. The difference in the ultimate unit stresses is around 3% in favor of the theoretical section modulus.

In this excellent paper, only Test No. 24 and Test No. 10(a) were with 2-in., wrought-iron pipes; but there is no value given for either of them under Column (14), Table 2, comparable with the writer's data for 5-in. pipe. It would be of interest if the authors could make this comparison possible.

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DISCUSSIONS

TENSION TESTS OF LARGE RIVETED JOINTS

Discussion

By A. E. Richard De Jonge, M. Am. Soc. C. E.

A. E. Richard de Jonge,[25] M. Am. Soc. C. E. (by letter).[25a]—The tests described in this paper are perhaps the most elaborate tension tests conducted to date on large-size structural riveted joints. A variety of specimens was tested, in which several characteristics were changed systematically. The questions that were to be answered by these tests are clearly stated under the eight items at the end of the "Introduction", and the answers are given in "Conclusions from the Tests".

After reading the lengthy report, the writer who has made an extensive study of the world's literature on riveted joints[26], would like to take stock of what has really been accomplished by these very expensive tests. In so doing, he is not going to discuss the eight items previously mentioned, but will rather deal with those factors that have an influence on the strength of riveted joints, in so far as they have been treated by the authors.

(*1*) *Strength of a Riveted Joint.*—The meaning of the term "strength of a riveted joint" should be revised so as to be in accordance with actual facts. In the past, too much emphasis has been placed on the ultimate strength of a riveted joint. With the modern means of investigation, there is no reason why the method introduced more than one hundred years ago by Sir William Fairbairn[27] should still be adhered to to-day. In service, no actual joint is ever stressed anywhere near the breaking point. Therefore, it matters little whether the joint, if stressed to the breaking point, fails in the rivets or in the plates; in both cases the joint becomes useless. In fact, if the rivets would fail before the plates become distorted to any great extent, it would be possible to

Note.—This paper by Raymond E. Davis and Glenn B. Woodruff, Members, Am. Soc. C. E., and Harmer E. Davis, Assoc. M. Am. Soc. C. E., was published in May, 1939, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: September, 1939, by Messrs. Charles F. Goodrich, Frederick P. Shearwood, and Jonathan Jones; and October, 1939, by Messrs. C. C. Winter, W. M. Wilson, and J. M. Garrelts.

[25] Adjunct Prof., Polytechnic Inst. of Brooklyn, Brooklyn, N. Y.

[25a] Received by the Secretary January 31, 1940.

[26] "Riveted Joints: A Critical Review of the Literature Covering Their Development, with Bibliography and Abstract from the Most Important Articles," by A. E. Richard de Jonge, *Research Publication*, A.S.M.E., 1940; see also *Transactions*, Am. Soc. C. E., Vol. 99 (1934), p. 474.

[27] "Experimental Inquiry into the Strength of Iron with Respect to Its Application as a Substitute for Wood in Shipbuilding," by William Fairbairn, British Association for the Advancement of Science, Tenth Meeting, Glasgow, Scotland, Reports 1840, Vol. 1840, pp. 201–202; see also *Philosophical Transactions*, Royal Soc. of London, Vol. 140, 1850, pp. 677–725.

re-rivet the joint, thereby making it again serviceable. This is impossible, however, when the joint fails in the plates.

Consequently, it is of far greater importance to determine what happens in a riveted joint up to the maximum working load than to pull the joint apart—that is, subject it to conditions to which, ordinarily, it would never be subjected in practice. The only reason for the ultimate strength test is to provide data for a convenient, although wholly inadequate, method of calculation which, by the introduction of a suitable factor of safety, assures sufficient safety at the maximum working load that the joint is called upon to sustain. Substituting for this "factor of ignorance" a thorough knowledge of the phenomena that occur in the joint, from the moment a load is applied to it until this load has reached the maximum value allowable under ordinary working conditions, would lead to a rational method of calculation which, at present, is sadly lacking. The maximum working load should remain sufficiently far below the yield strength of the riveted joint rather than below the ultimate strength. The yield strength would then form the true strength limit for the joint, which can ordinarily be utilized. However, since the authors have followed a long-established precedent[27] in subjecting riveted joints to breaking loads, a discussion of the various factors that control the strength of a joint, up to the point of fracture, will be necessary.

(*2*) *Tests of Solid Plates.*—The authors have subjected two solid plates each of Series C and D to tension tests. They were of carbon steel, and had a width of 21 in. and a gage length of 24 in., two being $\frac{3}{8}$ in. thick and the other two, $\frac{5}{8}$ in. thick. Marked yielding occurred in the $\frac{3}{8}$-in. plates at 37.9 kips per sq in., and in the $\frac{5}{8}$-in. plates at 34.8 kips per sq in. The $\frac{3}{8}$-in. and $\frac{5}{8}$-in. plates failed, respectively, at strengths of 92% and 94% of that of the test coupons. The ratios of lateral set to longitudinal set, at failure, were 0.38 for the $\frac{3}{8}$-in. plates and 0.40 for the $\frac{5}{8}$-in. plates. Poisson's ratio was found to be 0.28 for both plate thicknesses. The plates failed at mid-length. In three specimens, a short transverse crack started in the middle of the plate, whereas in the fourth the first crack developed at one edge. The authors measured the longitudinal strains at 30 kips per sq in. and found them to be uniform—that is, the strains were the same at the center as at the edges. In one $\frac{3}{8}$-in. plate, the strains were measured at 35 kips per sq in. (that is, very close to the yield point) and were found to be 11% greater in the middle than at the edges.

The authors attribute the tendency for the failure of the plates to start in the middle to the unequal distribution of longitudinal strains. To the writer, this seems to be correct, but it requires amplification to explain this behavior.

As long as the cross-contraction is approximately the same over the gage length, the strains are the same everywhere in the plate. A network of small squares scratched into the surface of the plate at the beginning would simply be changed into a network of rectangles. However, shortly before yielding becomes general, a slight necking occurs at the edges of the plate, usually at mid-length. This is an indication for a redistribution of the material. The thicker the plate, the easier this redistribution will proceed—that is, the more marked will the necking become. In the region of necking, the small squares are distorted into a kind of trapezoid with curved sides; but the orthogonal system of

tension and compression lines that coincides with the sides of the original squares must remain orthogonal. The greatest lengthening of the tension sides of these squares, therefore, takes place at the center of the plate in the region where necking at the edges occurs. This appears to be the reason why the first crack must start in the middle of the plate instead of at the edges. As the redistribution of the material takes place easier in thick than in thin plates, it is to be expected that the final set must be greater in thick than in thin plates. This is precisely what was observed. Why failure started at the edge, in one case, cannot be determined without further knowledge of the fracture. It seems likely that, due to a slight notch effect at the edge, the crack started from this notch.

Because the redistribution of the material is more difficult in thin plates than in thick plates and a round bar, and because the elongation of the $\frac{3}{8}$-in. plates is less than that of the $\frac{5}{8}$-in. plates, as observed by the authors, the ultimate strength exhibited by the $\frac{3}{8}$-in. plates did not reach that of the $\frac{5}{8}$-in. plates and was lower than that of the test coupons (92%), whereas the ultimate strength shown by the $\frac{5}{8}$-in. plates was somewhat higher, but still below that of the test coupons (94%). Thus, the foregoing explanation describes all the observed facts accurately. Furthermore, if a $\frac{3}{8}$-in. plate is bent into the shape of a cylinder (that is, if it is formed into a tube of $\frac{3}{8}$-in. wall thickness, with a lateral surface equal to the width of the original $\frac{3}{8}$-in. plate), and if it is then pulled apart, it will most likely be found that the strength approaches much closer that of the test coupons, because the entire wall of the tube can now contract together with the latter (necking) at right angles to the surface. If the plate should become a bar, the thickness of which is equal to its width, and if it were possible to tear it apart, it would probably be found that its strength would approach that of the test coupon very closely. To the writer, these appear to be the reasons underlying the strange behavior of wide plates. As the tests by the authors have revealed these facts, it can be said that these experiments have served a useful purpose.

(3) *Effective Net Section and Grooving Effect.*—Of effective net section, grooving effect, rivet pattern, omission of rivets in the outer rows, pitch, and gage-line distances—all of which factors are intimately interconnected—let the first two be discussed at this point.

The authors claim that "an outstanding finding of these tests is that the strength of the plates in a riveted joint is not directly proportional to the area of the net section as commonly assumed in practice". Theoretical and experimental studies on the net section have previously been conducted by C. R. Young[28], M. Am. Soc. C. E., and by Professor Young and W. C. Dunbar[29], in which it was found that the net section, assumed to be stressed uniformly, is not thus stressed, and is not a simple function of the rivet-hole arrangement. T. R. Loudon[30] also found, by using photo-elastic analysis, that there are high stress concentrations near the rivet holes, and that the stress is, by no means,

[28] (a) *Bulletin No. 2*, School of Eng. Research, Univ. of Toronto, 1921, pp. 233–244; (b) *Canadian Engineer*, Vol. 39, 1920, p. 427; (c) *ibid.*, pp. 512–513; and (d) *Engineering and Contracting*, Vol. 54, 1920, pp. 188–189.

[29] *Bulletin No. 6*, School of Eng. Research, Univ. of Toronto, 1926, pp. 51–65.

[30] Faculty for Applied Science and Eng., Univ. of Toronto, School of Eng. Research, *Bulletin No. 9*, 1932, pp. 231–239.

distributed uniformly over the section remaining between the rivet holes. Therefore, the authors can scarcely claim priority for their finding.

Probably the entire question of net section would long since have assumed a different aspect if the stress distribution, as found by Prof. E. G. Coker[31] and by Professor Loudon[30], had been considered more carefully; for a phenomenon, termed the "grooving effect," occurs in which the material in the vicinity of the rivet absorbs a greater part of the stresses than the material farther away so that the material between two closely spaced rivet holes exhibits an apparently greater strength. This phenomenon was first stated definitely by Daniel Adamson[32], in 1878, although, previously in 1872, Walter R. Browne[33] had shown that the stress distribution in the remaining cross-section is not uniform, the stress being greater near the hole. According to William Hovgaard[34], the material of mild steel plates, $\frac{3}{8}$ in. to $\frac{3}{4}$ in. thick, absorbs, between two rivets spaced at a gage of three times the diameter (3 *D*), a 10% higher stress than does the solid plate ahead of the rivets. This amount drops to 5% if the gage distance between rivets is four times the diameter (4 *D*). The grooving effect is of the utmost importance with regard to the rivet arrangement or rivet pattern, as will be shown subsequently.

(*4*) *Rivet Pattern, Omission of Rivets in the First Row, Pitch, and Gage-Line Distances.*—Only a few, and scarcely representative, rivet patterns were investigated by the authors. Long ago it has been shown that the omission of rivets in the first row, for the purpose of increasing the strength of riveted joints, is a fallacy[35] (Meldahl showed that, in the case of a saw-tooth like doubling plate, the maximum stress occurs slightly ahead of the ends of the saw-teeth, and that fracture actually started at those points); it is based on the misconception that the stress between the rivets of the first row is uniformly distributed (assumption by J. W. Schwedler[36]). Even as far back as 1860, Sir William Fairbairn[37] recommended, where greatest strength combined with lightest weight is desired in bridge girders, the use of chain riveting or placing the rivets in the various lines, one behind the other, instead of "in zigzag" as was customary at that time. (The writer, here, uses the term "rivets in line" for rivets placed longitudinally one after the other, and the term "row" for rivets spaced side by side transversely to the direction of the acting force.) The same recommendation was made again by Commander Gayhart, in 1926, on the basis of his very elaborate tests[6]. Gayhart showed conclusively that chain riveting (placing the rivets in line one after the other and using like lines, evenly spaced, side by side) is stronger than riveting in which, alternately, one

[31] *Transactions*, Inst. of Naval Architects, Vol. 53, Pt. 1, 1911, pp. 265–296, including discussion; see also "A Treatise on Photo-Elasticity," by E. G. Coker and L. N. G. Filon, Cambridge, 1931.

[32] "On the Mechanical and Other Properties of Iron and Mild Steel," by Daniel Adamson, *Journal*, Iron and Steel Inst., Vol. 1878, pp. 383–403.

[33] "Drilling Versus Punching," by Walter R. Browne, *Engineer*, Vol. 34, 1872, p. 362.

[34] "Structural Design of Warships," by William Hovgaard, E. and F. N. Spon, Ltd., Spon and Chamberlain, London, 1915, p. 121.

[35] "Materialspannungen in ausgeschnittenen Platten," by K. G. Meldahl, *Schiffbautechnische Gesellschaft*, Berlin, Jahrbuch, Vol. 5, 1904, pp. 481–523.

[36] "Über Nietverbindungen," by J. W. Schwedler, *Wochenschrift des Architekten Vereins zu Berlin*, Vol. 1, 1868, pp. 451–452, 463–464, and 472–473.

[37] "Chain Riveting," by William Fairbairn, *Mechanics' Magazine*, Vol. 3, 1860, pp. 375–376; see also *Journal*, Franklin Inst., Vol. 70, 1860, pp. 237–239.

[6] "An Investigation of the Behavior and of the Ultimate Strength of Riveted Joints Under Load," by E. L. Gayhart, *Transactions*, Soc. of Naval Architects and Marine Engrs., Vol. 34, 1926, p. 55.

or more rivets in the first row are omitted. Since this was well known at the time these tests were planned, the writer would have expected that the joints for these tests would have been laid out with due regard for this fact. The only specimens that approach fairly closely the chain riveting recommended by Fairbairn and Gayhart are those of Series B. However, due to the omission of two rivets in one of the inner rows of each specimen of this series, these specimens cannot be regarded as good examples of joints designed for greatest strength. Nevertheless, the tests proved that these joints had a better efficiency than other joints—for instance, those of Series A.

The reasons why full-row rivet spacing in the first row shows a greater strength are: (1) The grooving effect (the capacity of the material between narrowly spaced rivets to absorb greater stresses than the material between widely spaced rivets); and (2) the fact that the stress distribution in the case of narrow, even rivet spacing is far more uniform than that in the case of widely spaced rivets, as has been shown, for example, by Coker who used photo-elastic analysis[31]. Thus, by spacing the rivets in the outer rows evenly and closely, dangerous stress concentrations are avoided at these rivets.

Next, rivet pitch, particularly between the first two rows in the case where the rivets in these are spaced in zigzag, has been investigated by the authors, in Series C and D. In all cases, the fracture passed through the two outermost rivets and showed almost always a tendency to follow shear lines inclined at about 45 degrees. This tendency was greater in the $\frac{5}{8}$-in. plates than in the $\frac{3}{8}$-in. plates. Where the second row of rivets was near enough (Specimens CCC2–2 and DCC2–3), the fracture passed in zigzag through first and second row rivets[38]. An interesting feature is that, at yield load, Specimens CCC2–2 and CCC2–4 showed about the same efficiencies, whereas the specimens with the intermediate pitch of 3 in. (CCC2–3) exhibited considerably lower efficiencies. At ultimate loads, however, the efficiencies vary as shown in Table 7. In that case, the maximum efficiency for both $\frac{3}{8}$-in. and $\frac{5}{8}$-in. plates is reached for an end-row pitch of 4 in. For smaller, as well as for greater, pitches, the efficiency was lower.

The rivet pitch throughout the joint was the only variable that was changed in the specimens of Series B. The gage distance was rather large (7.5 in.), and thus, in the light of what has been said about the stress distribution, the joints cannot be regarded as being representative of best or even ordinary practice. Nevertheless, the results are of interest in so far as they show that joints with 4.5-in. pitch throughout were stronger than either those with 3-in., or those with 6-in., pitch (see Table 15). It is to be regretted that no attempt was made to vary the gage-line distances as this would have revealed the optimum condition of pitch relative to gage distance. It is obvious that, when the gage-line distance is too great, stress concentrations at the outer row of rivets will appear so that the stress distribution will become uneven, resulting in reduced strength.

The effect of the change in gage distance, or of the number of holes in a given section, may be judged, to a certain extent, from Specimens CCC5 and

[38] See also "Weerstand van de doorsnede van een plaat tuschen nagels in een scheeve rij, berekend volgens de breukhypothese van Mohr," by P. P. Bijlaard, *De Ingenieur*, Vol. 46, 1931, pp. 47–57.

DCC7. It should be kept in mind, however, that the former consisted of $\frac{3}{8}$-in. plates, whereas the latter were composed of $\frac{5}{8}$-in. plates. As may be expected from what has been said about the grooving effect, Specimens DCC7 with the narrow-gage distances of 3 in. sustained a greater load (more than 76.2 kips per sq in. of net section) than did Specimens CCC5 with the wider gage distances of 4 in. (70.9 kips per sq in.). The grooving effect of the narrow-gage distances proved sufficient to prevent failure in the plates in spite of the higher stress in the net section, the joints, in this case, failing in the rivets.

(*5*) *Bending in Lap Joints.*—In its essentials, the foregoing discussion, under Items (3) and (4), applies equally to lap joints and butt joints. The high bending stresses that usually occur in lap joints, particularly in short ones, were eliminated by the authors to a certain extent by the use of a "bending frame". While, thus, the values indicated in Table 5 are intended to represent data purely in tension without bending, it must be kept in mind that bending is always present in actual lap joints. The bending stresses that occur should be placed on the debit side in such joints. The values given by the authors, therefore, have only a theoretical interest and should be used with caution wherever bending stresses are present at the ends of the lap, particularly since the bending frame tended to increase the friction between the plates.

(*6*) *Efficiency of Lap Joints.*—In Table 5, the authors have given the efficiencies for lap joints that failed in the plates, as well as for those that failed in the rivets. The efficiencies of joints of Series C and D which failed in the plates vary from 72.8% to 78.8%, the mean being approximately 75 per cent. Considering the different types of rivet patterns, the different lengths of the laps, the different pitches and gage-line distances, and the different kinds of steel, it seems remarkable that the differences in the efficiencies were not very much greater. However, it should be emphasized again, that, for a joint used in actual construction, the ultimate efficiencies mean little; it is rather the efficiency up to, or at, the yield strength of the joint that is of importance and should so be considered by the designer.

(*7*) *Rivet Data.*—

(a) Driving Temperatures.—The strength of a riveted joint depends, to a large extent, on the clamping effect of the rivets, and this, in turn, on the temperatures at which the rivets were driven. Two temperatures are of importance—the one at which driving starts and the one at which it ends. The data given by the authors are extremely scanty. No definite temperatures are given. As to the temperature at which driving started, the authors give the range and average in a general way: For carbon-steel rivets from 1 400° F to 1 800° F, with an average of 1 570° F, and for manganese-steel rivets from 1 400° F to 1 900° F, with an average of 1 680° F. The far more important end temperatures at which the ram of the hydraulic riveter completes the forming of the snaphead, and that at which it leaves the rivet, are not stated; and yet, these have a deciding influence on the strength of the joint. Furthermore, no mention has been made as to how these temperatures were measured. It is hoped that the authors will give further information as to these important factors in their closing discussion.

An extensive investigation of the most suitable rivet temperatures at the beginning and end of driving was published by Yasuchi Taji[39], in 1914. Taji recommended the use of the lowest possible temperatures, with riveting done as quickly as possible. Although he used a wide range of temperatures at the start of driving the rivets, his general average, in hydraulic riveting, is 817° C (about 1 503° F). Although this average value is lower than that used by the authors, it is well within the range used by them. For the lower end temperature, Taji gives, as the best value, 650° C (about 1 200° F), and this is in remarkably good agreement with the optimum end temperatures given in 1886 by A. Considère[40], in France, who recommended an end temperature of from 600° to 700° C (about 1 100° to 1 300° F). If one considers that the average temperature used by Taji was almost 70° F lower than that used by the authors, it is questionable whether the end temperatures in the authors' case were close to the best end temperature recommended by Taji.

That better strength values may be obtained by using lower temperatures is borne out by some of the authors' specimens of Series A (manganese-steel rivets and double-plate joints). Without giving the actual temperature at the start of driving, the authors state that it was about 100° F less than in the other cases of this series; yet, the strength of these joints was found to be well above that of the other joints, and the rivets had a greater hardness and finer grain structure. Other investigators, too, have obtained better results at lower initial temperatures—for instance, Professors Young and Dunbar[41], whose grand average was 1 384° F. It is to be deplored that the authors did not follow up this clue by experimenting with somewhat lower temperatures, using joints of a definite type, but with rivets driven at different initial and final temperatures.

(b) Rivet Strength.—As stated under Item (7), the strength of a rivet and, thus, also of the joint depends on the temperatures at the beginning and end of driving. The important question, however, is what the actual strength of a driven rivet is as compared with that of the undriven rivet and the stock from which the rivet was made. The authors have given yield strength and ultimate tensile strength for the rivet-rod stock, and the shear strength for undriven rivets, but no actual shear strengths of driven rivets except the mean shear strengths as computed from the tests of the joints. In Tables 9, 10, 12, 13, 14, 15, 17, and 19, average shear stresses are given which are computed for effective rivet yield, yield load, and ultimate load. For the latter, the authors have stated that it was found "by dividing the total load by the total shearing area of rivets, based on the nominal diameter of the rivet". However, nothing has been mentioned about how the other two values were obtained. At the "effective rivet yield" and yield load, the shear is certainly not distributed uniformly over all the rivets of the joint. Therefore, it is not clear how the shear values in these cases were obtained. At ultimate loads, however, it seems likely that the shear is more or less uniformly distributed.

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[39] "Some Recent Studies on Rivets, Riveting, and Riveted Joints," by Yasuchi Taji, Soc. of Naval Architects and Marine Engrs., Zosen Kyokwai, Vol. 13, September, 1914, pp. 127–181.

[40] "Mémoire sur l'Emploi du Fer et de l'Acier dans les Constructions," by A. Considère, *II. Annales des Ponts et Chaussées*, 6me série, Vol. 2, 1er semestre, 1886, pp. 5–149.

[41] "Rivets in Combined Tension, Shear, and Flexure," by C. R. Young and W. C. Dunbar, *Canadian Engineer*, Vol. 56, 1929, pp. 201–206.

Furthermore, it should be kept in mind that the rivet-rod stock and the rivet produced from it by a heating process, if subjected to shear, are stressed more or less biaxially, whereas the driven rivet, which, due to contraction by cooling, is under tension, is stressed triaxially if subjected to additional shear. This explains the differences in the shear values for rivet-rod stock, undriven rivets, and driven rivets.

For the foregoing reasons, the values tabulated by the authors cannot be regarded as absolute values, but only as relative values for the purpose of making some kind of a comparison possible. Caution is urged, therefore, in using these values.

(c) Single Shear vs. Double Shear.—Under this heading, the authors mention in their "Conclusions from the Tests" that a value as low as 0.80 is indicated for the ratio of double shear per shearing surface to that of single shear. The authors state that this value agrees with findings from their pilot tests. It is not quite clear how this value was derived, because if one compares, for example, the ultimate shear values per shearing surface for Specimens FSCA and ASC18 (namely 41.6 and 54.8 kips per sq in., respectively) their ratio is 0.76, which is a still lower value and much lower than that found by other investigators. E. S. Fitzsimmons[42], for example, obtained, for carbon-steel rivets in double shear (per two shearing surfaces), 84 kips per sq in., and for like rivets in single shear, 49 kips per sq in., from which the value of the ratio of double shear per single surface to single shear follows as 0.857. Other investigators have found this ratio to be approximately 0.9. As the aforementioned specimens vary considerably in rivet pattern, rivet diameter, and width and thickness of plates, this comparison is, probably, not a fair one. It would have been of great value if the authors had conducted tests with otherwise similar specimens, in single and in double shear, with one and more rivets. In that case, reliance could be placed on an average value obtained thereby; but this, certainly, cannot be said about the value of 0.80 given by the authors. It should be mentioned, however, that, in their recommendations, the authors have used the value of 0.9, which they, too, probably consider as the most likely one.

(8) *Influence of Plate and Rivet Material.*—Two types of rivet steels were used in these tests—a carbon steel and a manganese steel. The tensile strength of the rivet-rod stock is given by the authors, for $\frac{7}{8}$-in. and 1-in. carbon-steel rods, and for the 1-in manganese-steel rod, as 58, 57.2, and 81 kips per sq in., respectively. The great loss in tensile strength of the annealed manganese steel, to 74.7 kips per sq in., should be noted. It is to be regretted that the data on these steels are very fragmentary. In the first place, the shearing strengths in single and double shear of the rivet-rod stock, as stated, are not given; neither are the moduli of elasticity and shear, nor the relationship between elongation and stress. For "non-driven rivets", the shearing strength in double shear (probably per single shearing surface) is given for 1-in. carbon-steel and 1-in. manganese-steel rivets, but the shearing strength in single shear is not stated. If the maximum shearing strength (in single shear) is assumed to be 0.8 of the ultimate tensile strength of the rivet-rod stock (a factor which,

[42] "The Shearing Strength of Rivets," by E. S. Fitzsimmons, *Iron Age*, Vol. 75, 1905, p. 2058.

usually, is not far from the actual), the shearing strength in single shear, for the carbon-steel and manganese-steel 1-in. rivets, would be obtained as about 45.8 and 64.8 kips per sq in., respectively. The ratios of double shear per single shearing surface to single shear would then follow as $\frac{42.1}{45.8} = 0.92$ and $\frac{54.5}{64.8} = 0.84$, respectively—values which are more in accord with those of other investigators as stated previously herein. In double shear, the undriven manganese-steel rivets were a little more than 29% stronger than the carbon-steel rivets. If the ultimate shearing strength of Specimens ACC–18 and ACM–12 is compared, it is found that the mean in the case of the former was 55.7 and in that of the latter 75.9 kips per sq in. Hence, in these joints, the manganese-steel rivets proved to be about 36% stronger than the carbon-steel rivets. However, one must not lose sight of the fact that the joint with carbon-steel rivets was longer than that with manganese-steel rivets, the plates, in both cases, being of carbon steel.

As regards the three kinds of steel used in the various joints, the authors have failed to state the exact ultimate strength of the plates used in each joint. Likewise, the moduli of elasticity and shear are not given and neither are the stress-strain relations. Thus, the checking of the elastic properties of the joints is made impossible. Unless the authors supply these values in their closing remarks, this entire set of data is useless from the point of view of developing a rational method of calculating riveted joints. The statement that the effect of the plate steel in joints which failed in the rivets was neither large nor consistent, although the decrease in shearing strength of the rivets with length was somewhat more marked in the case of the carbon-steel plates than with less ductile steels, should be questioned. One would have expected that the harder silicon steel would exert a stronger cutting action, a fact which is actually borne out by a comparison of Specimens ASC–18 and ACC–18. In the former, the average ultimate shearing value was 54.8 kips per sq in., whereas in the latter, it was 55.7 kips per sq in., thus showing that the shearing strength of the rivets in the case of silicon-steel plates is slightly less than that in the case of carbon-steel plates. This holds even for plates with manganese-steel rivets, for, in comparing the respective shearing values for Specimens ASM–12 and ACM–12 (namely, 72.4 and 75.9 kips per sq in.), the shearing strength of the rivets in these joints with silicon-steel plates is found to be somewhat lower than that of the joints with carbon plates. In general, the combination of nickel-steel plates and manganese-steel rivets seems to be better than that of silicon-steel plates and manganese-steel rivets. In order to arrive at a decision as to which combination is the best under a given set of conditions, further experiments appear to be necessary which should be made with the strongest joints that can be designed, keeping in mind the facts dealt with in this discussion.

(9) *Effect of Length of Joints and Premature Rivet Failure.*—In general, the authors have described, correctly, the effect of length of a joint. As is well known from former tests and theoretical considerations, the strength of a riveted joint decreases with its length[43]. It stands to reason that the longer the joint,

[43] "Inanspruchnahme der Anschlussnieten elastischer Stäbe," by Ivan Arnovljević, *Zeitschrift für Architektur und Ingenieurwesen*, Vol. 14, 1909, pp. 89–106.

the greater will be the end slip and the more likely will the rivets in the end rows shear off prematurely—that is, before ultimate failure of the joint. The more ductile the steel of the plates, the greater will be the elongation and slip, and the more readily will the end rivets be sheared off. Therefore, this tendency is greatest in carbon-steel plates, and less in silicon-steel and nickel-steel plates. For the latter, in combination with manganese-steel rivets, the authors did not observe any such tendency.

A fact not discussed by the authors is that, when the end rivets failed, the joint with the remaining rivets still proved strong enough to withstand the full load. As the outer rivets failed almost consistently, it is obvious that they carried a greater load than the others. On the other hand, the next row, or the over-next row when the rivets in the two or three outer rows failed, made the plates (on account of the closer rivet spacing in the subsequent rows) relatively stronger due to the grooving effect, and the stressing of the remaining rivets became more uniform due to the more uniform stressing of the plates. Thus, one cannot escape the conclusion that the rivets in the outer row (or rows) were unnecessary for developing the maximum strength exhibited by the joint. This is a further proof for the fact that, even at ultimate loads, joints with closer rivet spacing in the outer rows are stronger both in the plates and in the rivets. That the transverse contraction of the plates increases the relative displacement of the rivet holes in adjoining plates is obvious. However, the authors' statement that "excessive detrusion, rather than stress, caused such failures," cannot be accepted by the writer as the actual stress always increases until failure occurs. Unfortunately, engineers accept the usual stress-strain curves, in which the curve, after reaching a maximum, drops again, without being conscious of the fact that these curves are, for convenience, referred to the original cross-section of the test bar instead of (as they should be) to the smallest cross-section in the region of necking. If the curves were thus re-plotted, it would be found that they would rise continuously up to the point where fracture finally occurs. Therefore, the statement that these outer rivets failed by overstressing is fully justified, the so-called theory of limit design to the contrary notwithstanding[44].

(*10*) *Butt and Shingle Joints.*—The efficiencies of the joints of Series F showed that the short butt joints had the highest efficiencies—75.2 per cent. Comparing the double-plate butt joints with the double-plate shingle joints (the respective efficiencies of which are 73.0% and 72.1%), it will be seen that the efficiency of the butt joints is slightly higher, as it should be according to theory[43, 45]. As is to be expected, the longer joints showed the lower efficiencies. The authors' statement that the advantage of the lighter shingle and half-shingle splices, compared with the heavier butt splices, is offset by greater difficulty in field erection should be noted. The triple-plate, half-shingle joint (Fig. 16(*b*)) does not seem to have been designed correctly, for it fractured, as one would expect, near the unsymmetrically arranged butt of the center plate. The authors state that the outer splice plates in the triple-plate

[44] "Theory of Limit Design," by J. A. Van den Broek, M. Am. Soc. C. E., *Proceedings*, Am. Soc. C. E., February, 1939, pp. 193–216, particularly p. 195.

[45] "Zur Kraftverteilung in genieteten Stäben," by Ivan Arnovļević, *Oesterreichische Wochenschrift für den öffentlichen Baudienst*, Vol. 14, 1908, pp. 607–615.

butt joint (Fig. 15) were shorter than had been intended, and the twenty-seven rivets that passed through inner and outer splice plates transferred 61% of the load from the main plate to the splice plates, whereas the thirty rivets that passed through only the inner splice plates transferred but 39% of the load. The question should, here, be raised as to what this load was—whether it was the ultimate load or some other load—as the load distribution varies with the load. The authors have failed to state this clearly. In explanation of the somewhat unexpected load distribution in these joints, it may be said that the longer rivets were probably under a higher tension than the shorter ones, and that, for this reason, they exerted a greater clamping pressure on the plates. The friction, which, up to the time of fracture, undoubtedly played an important part, was probably the cause of this rather strange distribution of load. The same explanation holds also for the double-plate butt joints, although to a lesser extent.

The authors have undertaken extensive strain measurements on specimens of this Series F. It is to be regretted very much that the results of these measurements were not presented in tabulated form to enable the investigator to check the results by calculation. Apparently, what the authors have done (although they have not stated this clearly) is to compute the stresses in the net section of the various plates for different loads (7.5, 15, 22.5, and 30 kips per sq in., or 15 and 30 kips per sq in.) and then to average them and give them as percentages in the diagrams above the joints[45a]. If this understanding is correct, it must be stated that the curves given are of little value since the load distribution and, consequently, the strains and stresses vary with the load. In fact, it would have been better if the authors had given the curves for one definite load only, as they would then be available for checking eventual calculations that may be derived. At any rate, the authors' statement that the end rivets and those near the butt have taken a greater load than the remaining ones is significant.

(*11*) *Behavior of Riveted Joints.*—Last, but by no means least, elongation, slip, distribution of strains, and partition of load—in short, the behavior of riveted joints under load—will be discussed. All these factors are intimately interconnected, but are dealt with separately herein.

(a) Elongation.—Unfortunately, the authors have not given a definition of what they wish to be understood by elongation. In lap joints, it is either the change in length of the distance between the outer ends of the plates, or the change in length between the points on the main plates, coinciding at the beginning with the outer ends of the adjoining plates. Similarly, in butt joints, elongation is either the change in length of the splice plates, or the change in distance between the points on the main plates coinciding at the beginning with the ends of the splice plates. In other words, the authors should have stated if, by elongation, they mean the elastic and plastic change in length of the plates between their ends, without the end slip, or if the elongation is to include the end slip at either end. This makes quite a difference. A clear-cut definition would have helped greatly in gaining a correct understanding.

[45a] See Corrections for *Transactions*, in *Proceedings*, Am. Soc. C. E., September, 1939, p. 1304.

Furthermore, the authors use a ratio of unit elongation of the joint to unit elongation of the gross-section of the main plates, which, for lap joints, is shown (see Table 9) to vary from 1.03 to 3.32—that is, within wide limits. The actual law of elongation has not been discussed. However, as an "effective modulus of elasticity" for the joints of Series F, the authors give a value of about 29 000 000 lb per sq in. as against 32 000 000 lb per sq in. for the main plates. For this series, the elongation ratio varied from 0.79 to 1.13 which is about 43% on the basis of the lower, and about 30% on the basis of the higher value (see Table 17, Column (12)). The writer is unable to understand how this effective modulus of elasticity takes care of this range.

(b) Slip.—The authors also have failed to define clearly what they mean by "slip", or, rather, they have been using the term in two very different senses: (1) For the movement of an entire plate relative to another plate; and (2) for the movement of parts of one plate relative to parts of another while other parts of both plates do not move relative to one another. At places, the authors have called the former "middle slip" and the latter "end slip". The way in which the latter was measured is not beyond reproach, as the locations vary somewhat. In almost all cases, the measurements were taken in the joint somewhere between the first and second rows of rivets. Only in the short specimens, ANM–12, were they measured in the first row of rivets. However, slip occurs even ahead of the first row of rivets, and the proper position for measuring the end slip would have been directly at the end of the plates, or of the splices. It is unfortunate that this was not done as it becomes, thereby, almost impossible to compare, directly, the measured results with those derived from a rational method of calculation. The middle slip is used by the authors as an indication that all rivets have come into full bearing, and they have plotted curves of the ratio of end slip to middle slip. The latter, however, mean little since this ratio combines two different effects in it as has been shown previously herein. Unfortunately, the authors have given only typical load-slip curves and have not entered their test points in these diagrams, from which fact it may be inferred that these are idealized curves, or, at least, curves of averages. As the test points were omitted in the diagrams of the load-slip relationship, the investigator is not able to form his own judgment as to whether the curves shown are actually the best that can be passed through the test points, or if local anomalies exist, and, if so, at what loads. Apparently, the curves are faired (almost too much so), a fact that does not make them appear at all likely where sudden slips occur. The true test points, or better still, a tabulation of the test readings, would have been of far greater value for comparisons between tests and calculations.

The load-slip relationship of the joints (representing their load-elongation diagram, which is akin to the stress-strain curves) has been divided by the authors into four stages. With the exception of the first, these stages have been described more or less accurately. Regarding the exception, they state: "In the first stage, static friction prevents slip." This is incorrect if it refers to end slip, but is correct if it refers to middle slip, which shows clearly the ambiguity of the term "slip" as used in the paper.

Of interest are the authors' statements regarding sudden short slips and snaps which occurred in Series F. The writer believes that it would be much appreciated if one could see some of the actual load-slip curves (including all test points) which exhibit these short slips.

The authors are partly correct in stating that the slip has little influence on the elongation. This statement holds true for the later stages; but it does, or does not, hold true for the last part of the first stage, according to the definition of elongation chosen. That the more ductile steels exhibit less middle slip than do the harder steels is well known, particularly from shipbuilding. The unknown fact, however, is how much the slip actually is; and this depends on the friction factor, the state of the faying surfaces (which is not stated by the authors), the pressure exerted by the rivets, the extent to which, and intensity with which, this pressure is distributed over the faying surfaces, the clearances between rivet shank and hole, etc.

For evolving a rational method of computation, the friction factor and distribution of the rivet pressure over the faying surfaces are absolutely essential. The coefficient of friction is required for various kinds of steel plates, in single-sided friction (single shear) as well as in double-sided friction (double shear), and for a single rivet, a single rivet row, and double rows of rivets. Before the present tests were started, the writer had requested that, at least, some tests of single rivet lap and butt joints be included. It is unfortunate that this was not done. The distribution of the rivet pressure over the faying surfaces is of importance as the friction factor depends on it. This distribution varies considerably with the thickness of the plates.

Of interest, also, is the statement by the authors that, in the lap joints of Series A, the working load of the rivets, for most joints, fell in Stage II, but that for nickel-steel joints with manganese-steel rivets, it fell in Stage III. Hence, it seems as if such nickel-steel joints might be calculated according to the elastic theory only (Ivan Arnovljević, Cyril Batho, and Alexander Hrennikoff, Assoc. M. Am. Soc. C. E.). Another interesting fact may be mentioned—namely, that in butt joints the end slip proved to be about three-fourths of that of lap joints of similar proportions.

(c) Distribution of Strain.—The authors are to be commended for having attempted the arduous task of establishing the strains and, thereby, also the stresses over the full width of the main plates in the joint for, at least, a few joints. The picture revealed thereby is illuminating. It shows that, with the exception of the area near the rivet holes, the longitudinal strains are distributed fairly uniformly across the joint. There is only one drawback to the results obtained—namely, that they were derived by the use of a rather long gage length (2.5 in.). For this reason, the resulting strains are more or less average strains between rivet rows. Since the strains and stresses vary sharply from point to point near the rivet holes, the authors could not possibly have detected the actual strain distribution in the vicinity of the holes with the strain gages they were using. How the distribution of the strains and stresses around a rivet hole, filled by a rivet and transmitting forces from the rivet to the plate, or *vice versa*, actually looks may be seen from the data published

by A. Hennig[46]. It would have been very interesting, in the light of what has been written previously herein about the grooving effect, if the authors had also determined the variation of the strains from point to point in a line along a rivet row—that is, transversely between rivets. However, extensometers of extremely short gage length would have had to be used in that case to obtain any accuracy at all.

From the diagrams of Fig. 11, however, it appears as if the strains have not been plotted correctly; or, rather, that the curves drawn through the plotted points do not represent the strains correctly. For example, in the diagram at the extreme upper left-hand corner of Fig. 11, the face strain labeled 1, starting from the left end, should remain constant in Plate 1 until, at least, the edge of Plate 2′. From then on, it should decrease to the first gage point between the first left-hand row of rivets and the second row, and, thereupon, follow the full curve shown by the authors, to its right end, from where the strain should drop to zero at the right-hand edge of Plate 1. The same holds true for the face strains in Plate 2′, only in reverse order. It appears that the authors have not used a sufficient number of gage points to obtain the true distribution. This also holds for the other two sets of curves in Fig. 11, but, for Specimen ACC54–1, not to quite the same extent.

If the strain, however, should actually have varied in the manner shown by the authors' curves, it would have been an indication of the fact that bending strains were present and, in that case, the strain on the opposite face of Plate 1, or of Plate 2′, respectively, should have been determined in order to eliminate the bending strains. Perhaps the authors can explain the course of the curves by giving further data.

In addition, the authors have measured the edge strains at the center of the plate edges. Their plots show that these strains do not differ too greatly from the average of the face strains, although, at places, the deviation is considerable, at least in the specimens of Series A to which Fig. 11 applies. Here, again, the long gage length used is an objectionable feature. As regards the course of the curves, remarks similar to the foregoing apply. The observations as to gage-line length apply also to the joints of Series F, for which the authors have, likewise, determined the strains, although at the edges only. More data would certainly be required for careful checks, and would be welcomed warmly.

(d) Partition of Load Among Rivets.—From the measurements of the strains, the authors have determined the distribution of the load over the various rivet rows (Fig. 13). These do not seem to follow the elastic theory closely. On the other hand, if the strains plotted by the authors in Fig. 11(*a*) are used for determining the distribution of the load, as was done by the writer, one arrives at a curve (for 20 kips per sq in. in the net section of the plate) which differs from the curves of load partition plotted by the authors in the lower left-hand corner of Fig. 13, and which represents more closely the load distribution curves derived from the elastic theory. This is particularly true for the curve derived from the face strains. Unfortunately, this is the only check that can be made with the material given by the authors. It would be

[46] "Polarisationsoptische Spannungsuntersuchungen am gelochten Zugstab und am Nietloch," by A. Hennig, *Forschung auf dem Gebiete des Ingenieurwesens*, Vol. 4, 1933, pp. 53–63.

very desirable if further material could be published by them so as to make the tests useful to the profession.

For Series F, the authors have determined the load carried by each plate at the various points. These curves were derived from the edge strains. As has already been stated, the gage length was, apparently, too great to give accurate results. As regards the load for which the curves were plotted, this has already been discussed under Item (10) herein. Fig. 14(*b*) shows, in Diagram C, that the end rivets, apparently, take a larger share than the other rivets, but that the load in the main plate increases again at about the third outer rivet row, after it has dropped about 3% more between the second and third rivet rows. This seems, indeed, very unlikely, because it means that the load carried by the splice plates would, again, be transferred to the main plates, a fact which could only occur in the case where the splice plates are considerably more rigid than the main plates. An explanation by the authors of this behavior, which does not appear in any of the other joints (at least not at the first few outer rivet rows) would be welcomed. In the shingle joints, however, it is true that load from one main plate is first transferred to the splice plates and is then again transferred from the splice plates to the main plates. Without having more accurate data than the curves which are shown at a rather small scale, it is not possible to check the behavior of these types of joints.

(*12*) *General Remarks and Summary.*—In summing up the foregoing, the writer makes the following general observations:

The present procedure of designing riveted joints is antiquated. It dates back more than 100 years when Fairbairn, in 1838, on the basis of his first practical tests, suggested the use of the ultimate strength of a riveted joint, in conjunction with a suitable factor of safety, as the criterion for its strength at working loads. From this procedure, no progress has been made within the last century, and the enormous sums spent for testing specific joints have produced, as the only result, an enormous accumulation of incoherent data that have never been properly correlated. The fault of this procedure has been recognized by some far-sighted minds, but they have not been able to replace it by a better one. This is due to the problem never having been attacked properly, a fact that was revealed to the writer by the study of the world's literature on riveted joints[26]. The deeper reason for it is that every one dealing with this problem has attempted to generalize (extrapolate) from individual experiments instead of to follow the opposite "path" of investigating, first, the underlying general and basic laws, and of particularizing (interpolating), then, by applying them to the specific problem or joint under consideration. The numerous and costly tests conducted to date might then have served as valuable checks and would have quickly revealed either the correctness or incorrectness of the theory, or its weaknesses.

Although the authors, obviously, had the best intentions of making these tests a basis for obtaining useful information and a clear insight into the behavior of riveted joints, very little has been accomplished that was not known previously, as has been shown herein. Therefore, these tests must be regarded as having, primarily, added further statistical material, obtained at ultimate loads, to the already enormous amount of such statistical information available.

The load-slip relationship has been cleared up somewhat, but the published data are insufficient to permit any rational theory of riveted joints, that may be devised, to be checked by them. The tests have failed to produce those basic data which are necessary for establishing a rational method of calculating riveted joints. No such method has been published as yet. Unless such a method is established by logical reasoning, which will give results that agree with practical measurements, it cannot be said that the problem of riveted joints has been solved completely.

The present method of calculation should be revised, at least so as to make the yield load the deciding factor and to give more weight to the behavior of riveted joints at working loads. In effect, this is a procedure similar to that recommended in the theory of limit design[44].

As regards the establishment of a rational theory of riveted joints, the following should be kept in mind:

Friction, which is frequently regarded as of doubtful value, provides the supporting force in most structures. Bearing of the rivets against the plates occurs only under exceptional conditions. In ships, for example, which, in heavy seas, are subjected to constant reversals of forces—that is, to alternating stressing—rivets, when cut out, seldom show any evidence of bearing; and yet, the joints of ships creak and squeak in heavy seas, an evidence of slip occurring under friction. No deterioration of the plate material on the faying surfaces has been observed, however. Riveted plate girders with splice plates in the web scarcely ever show evidence of the rivets bearing against the plates. They are held together rigidly by friction, as has been proved by tests. These are only a few examples showing the importance of friction as the supporting force. For this reason, friction in riveted joints should be studied extensively and far more than has been done in the past.

No information is available on how the rivet force used for clamping plates together spreads in these plates, nor how far its influence extends over the faying surfaces—that is, where the quilting action, or the lifting off of one plate from the other, starts. It is obvious that this influence depends on the rivet diameter, the diameter of the rivet head, and the thicknesses of the plates clamped together. To prevent over-stressing of the plates, the smallest possible pressure of the ram of the hydraulic riveter should be used, just sufficient to form the head properly, and the ram should be held on the finished rivet until it has cooled sufficiently in the shank to prevent plastic deformations in it during further cooling. Thus, the uncertainty of the rivet clamping force would be largely eliminated. The holes and the rivet shank in finished joints should be measured so as to obtain accurate information as to the clearance space between rivet and hole. Thus, basic data would be supplied for making a rational calculation of riveted joints possible.

As to specific recommendations, the writer offers the following, of which some coincide with similar ones made by the authors:

1. Riveted joint design should be based on the stress distribution along the joint, produced by the longitudinal forces, and particular emphasis should be placed on the stress peaks near the rivet holes;

2. Riveted joint design should be based, furthermore, on the distribution of the rivet clamping force (perpendicular to the plates), with particular emphasis on the extent to which, and the intensity with which, the rivet force spreads over the faying surfaces;

3. Chain riveting (full-row riveting) should be adopted generally;

4. Rivet pitch and gage-line distances depend on both the rivet diameter and the plate thickness;

5. Friction coefficients for various plate and rivet materials at rivet pressures should be determined;

6. Riveted joints should be laid out with reference to the yield strength on which working stresses should be based;

7. Riveted joints should be as short as possible (butt rather than shingle type); and

8. In general, carbon-steel rivets are more satisfactory than rivets of alloy steels except where it is necessary to save as much in weight as possible.

Corrections for *Transactions*: In Fig. 11(*c*), "Legend": Change "Face Strain" to "Edge Strain"; all the strains in the upper curves are edge strains; see also *Proceedings*, Am. Soc. C. E., September, 1939, p. 1304.

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DISCUSSIONS

THE UNIT HYDROGRAPH PRINCIPLE APPLIED TO SMALL WATER-SHEDS

Discussion

BY WALDO E. SMITH, M. AM. SOC. C. E.

WALDO E. SMITH,[13] M. AM. SOC. C. E. (by letter).[13a]—The author is to be commended for undertaking the task of determining distribution graphs for twenty-two small water-sheds, attempting to analyze the results, and to demonstrate applications. He has shown that the unit hydrograph principle is applicable to small water-sheds, as well as to large ones, with probably about the same general limitations. The distribution graph has justly become recognized as a handy tool in the kit of the hydrologist; but it should never be overlooked that the method is inherently an approximate one. At all times one should be aware of its shortcomings and inconsistencies, and should not expect too much refinement from it. For instance, it is self-evident that a tank of given size and outlet requires a longer time to become empty if it is full than if it is half full. It is likewise reasonable to expect that a channel full of surface run-off will require a longer time to be drawn down to ground-water flow than a channel only partly full; yet the width of base of the distribution graph is taken as constant for all cases, for a given water-shed. Similar deductions (some compensating to an unknown degree) can be made if ground-water flow is large or small, or if the average stream velocity is small because of overbank flow, or high because the stream is just at bankfull stage.

The writer is unable to share the author's optimism with regard to the conclusiveness of all of the points stated in the "Summary," particularly the items referring to the relation, or lack of relation, of duration of rainfall to the period of rise. Possibly it is within the inherent accuracy of the method, on small water-sheds, to assume that within the period of rise the duration of rainfall has no effect upon the shape of the resulting hydrograph. For large water-sheds, it is known that this cannot be done. However, to the writer, it does not appear that this assumption can be made for water-sheds of the size under

NOTE.—This paper by E. F. Brater, Jun. Am. Soc. C. E., was published in September, 1939, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: January, 1940, by Franklin F. Snyder, Jun. Am. Soc. C. E.; and February, 1940, by Messrs. LeRoy K. Sherman and Raphael G. Kazmann.

[13] Hydr. Engr., New Philadelphia, Ohio.

[13a] Received by the Secretary January 31, 1940.

discussion, either in the instance of the author's data or in the case of other similar data that the writer has available. If rainfall of uniform intensity in excess of the infiltration rates continues for the duration of the distribution graph, the period of rise will be found to be about equal to the duration or base of the distribution graph. Perhaps the best test of the validity of the author's assumption on the basis of his data is a consideration of the period of rise. Mr. Brater does not discuss variations in this value, but if one may use values obtained as accurately as possible from the distribution graphs of Figs. 3 and 4(*b*), then Tables 3(*b*) and 3(*c*) may be extended as shown in Table 6 (rearranging in the order of duration of precipitation).

TABLE 6.—Suggested Extension of Tables 3(*b*) and 3(*c*)

Date	Total rainfall	Duration of rainfall, in minutes	Period of rise, in minutes	Date	Total rainfall	Duration of rainfall, in minutes	Period of rise, in minutes
(*b*) Copper Basin Water-Shed; Stream No. 1; P = 38 Min				(*c*) Coweeta Water-Shed; Stream No. 9; P = 75 Min			
June 8, 1935	0.69	25	38	August 24, 1936	1.83	20	70
July 5, 1935	0.80	30	38	August 19, 1936	0.51	25	60
June 21, 1935	1.51	55	56	July 12, 1936	1.53	35	45
May 12, 1936	1.94	60	48	April 24, 1935	0.99	42	110
....	....	....	....	August 28, 1936	2.75	75	110
Average	....	....	45	Average	....	....	79

For the Copper Basin water-shed there seems to be, almost, a surprizing relation between duration of rainfall and period of rise; but if one examined only the first three of the Coweeta group, one might deduce that the period of rise decreases as the duration of rainfall increases. It would have been interesting if data on mass curves of rainfall and isohyetal maps for these storms and areas had been included in the paper. It is possible that in the case of the 35-min rain on the Coweeta water-shed, the major portion fell early in the period, or perhaps was heavier near the gaging station; but inasmuch as there is nearly a 150% variation from the minimum to the maximum period of rise, it is obviously affected by various factors. The statement concluding "Characteristics of the Distribution Graphs" is an example of the pitfalls into which one can be led by deductive reasoning. It is reasonable and rational, but it does not involve all factors.

In spite of the summary statements on lack of effect of duration of rainfall within the period of rise, under "Application of the Pluviagraph" the author has acknowledged that for the production of the pluviagraph in which "each unit storm produces a unit hydrograph," the precipitation must be divided into periods shorter than the period of rise, and "that a 30-min interval was too long * * * whereas a 15-min interval produced satisfactory results" for Copper Basin 2, with a given period of 60 min. If the author's statement that duration of rainfall within the period of rise has no effect is correct, it should be possible to put the 60-min rainfall into one of the four 15-min periods into which it was divided without effect upon the resulting pluviagraph.

The author has failed to give the lapse of time between the beginning of rainfall and the beginning of the rise. The period of the distribution graph should start with the time at which run-off producing rain starts. Is duration of rainfall, as given, the total time or only that period which presumably produced run-off?

The writer regrets that in engineering and hydrologic literature, distribution graphs are published so often, and that tables of distribution coefficients appear so seldom. The author has set forth coefficients in Table 2, for which he receives hearty approval; but Figs. 3 and 4 use the graph. The writer has found that he can determine a composite from several sets of distribution coefficients in tabular form much more readily than from their graph form, as Table 7 shows. Furthermore, a distribution graph as such has no direct use

TABLE 7.—SUMMARY OF DISTRIBUTION COEFFICIENTS FOR 12-HOUR PERIODS; LICKING RIVER AT TOBOSO, OHIO; 672 SQUARE MILES

Date	Precipitation*	Surface run-off*	Distribution Coefficients, by Periods, in Percentage of Surface Run-Off													
			1	2	3	4	5	6	7	8	9	10	11	12	13	14
(1935)																
August 6	....	....	0.0	15.0	28.0	27.0	14.0	8.0	4.0	2.0	1.0	1.0	..	..	..	..
(1937)																
January 14	1.58	1.09	0.0	18.0	32.0	25.0	8.0	6.0	3.0	2.0	2.0	2.0	1.0	1.0	..	..
January 17	0.73	0.58	1.0	26.0	34.0	14.0	7.0	5.0	3.0	3.0	2.0	2.0	1.0	1.0	1.0	..
January 21	1.58	1.03	1.0	22.0	33.0	17.0	8.0	5.0	4.0	3.0	2.0	2.0	1.0	1.0	1.0	..
January 24	1.76	1.45	1.0	22.0	31.0	19.0	8.0	4.0	3.0	3.0	2.0	2.0	2.0	1.0	1.0	1.0
June 21	3.73†	2.11	6.4	25.6	30.0	17.8	8.2	5.0	3.7	2.5	0.8	..	..	..	..	..
December 17	2.65‡	0.59	4.9	25.8	30.1	19.2	10.2	5.0	2.4	1.3	0.7	0.4	..	..	..	..
(1938)																
March 13	1.64§	0.88	7.7	25.7	31.7	19.8	6.9	4.3	2.1	1.2	0.6	..	..	..	..	..
April 6	1.56‖	0.83	1.3	24.2	33.7	17.4	10.0	8.2	3.9	1.3	..	..	..	..	..	..
(1939)																
January 29	1.59‖	0.80	2.7	24.8	31.0	23.1	10.1	4.5	2.0	1.1	0.6	..	..	..	..	..
Average	....	....	2.6	22.9	31.5	19.9	9.1	5.5	3.1	2.0	1.2	0.9	0.5	0.4	0.3	0.1

* In inches. † Precipitation heaviest in the south-southeast. ‡ Precipitation heaviest in the southeast. § Precipitation heaviest in the central part. ‖ Precipitation somewhat heavier in the north.

unless it is in block form as suggested by William T. Collins,[14] Jun. Am. Soc. C. E. Frequently it is plotted as a line connecting the points representing the coefficients, in which case its shape and consequent physical significance are lost.

Fig. 6 is particularly good in demonstrating, within a given region and general set of conditions, the effect of size upon the percentage of peak surface run-off. Fig. 7 is likewise very good; but as the author suggests, the values plotted involve a personal element in the selection of the terminal point of surface run-off and the resultant width of base of the distribution graph. The graph shows the probable trend of the effect of cover, but because so few data are given on factors such as slope and shape, it is not conclusive.

Mr. Brater would have done well to have extended his studies for Fig. 9 one step further and made determinations of infiltration rates. His statement

[14] "Runoff Distribution Graphs from Precipitation Occurring in More Than One Time Unit," by William T. Collins, *Civil Engineering*, September, 1939, Vol. 9, p. 559.

under "Run-Off Coefficients and Infiltration Capacity"—"Graphs such as these suggest that the pluviagraph provides a means of tracing the quantitative changes in infiltration capacity [rate] * * * during continuous rainfall * * *" —is the most noteworthy in the paper. Had the durations of the successive rains (in excess of the infiltration rate) been given, with quantity of rainfall for the same periods, the reader would have been enabled to do this for himself. It is possible that rainfall intensities in some of the cases cited were just in excess of infiltration rates, resulting in the small percentages of run-off noted.

The reference to the shape of the curves of Fig. 9, as a key to the solution of water-shed problems in general, calls to mind a study[15] which showed that coefficients for a given small water-shed, plotted against a quantity of rainfall, scatter over a considerable area.

As for the use of the distribution graph for forecasting flood flows, this is already being done extensively for primary water-sheds and for sub-water-sheds in connection with the operation of the Muskingum water-shed flood-control reservoirs in Ohio, as well as in the Tennessee Valley, the State of Pennsylvania,[16] and elsewhere. Depletion curves of ground-water flow are in use in many offices[17] for determining ground-water flow during periods of run-off, as well as between rains.

On various water-sheds, there is about every conceivable variation of size, slope, soil type, soil profile, soil condition, drainage density, cover, and a real distribution and intensity of rainfall, many of which factors are variable with regard to time as well as with regard to location. These factors are further thrown together in an infinite number of combinations. As far as empirical means are concerned, in addition to the unit graph method and as adjuncts to it, the methods described by R. L. Gregory and C. E. Arnold,[18] Assoc. Members, Am. Soc. C. E., by W. W. Horner, M. Am. Soc. C. E., and F. L. Flynt,[19] Assoc. M. Am. Soc. C. E., and others, are helpful.

Attempts have been made to determine, by analysis of the basin, the influence of each factor; but the great difficulty is that it is not possible to operate (as in a laboratory) to hold all variables constant, except one, and to study the effects of each one in turn. Possibly the best hope for an analytical solution is to plot run-off data with different slopes, soil types, cover, etc., each plot being a relatively homogeneous, small area; and then, after determining the extent to which each factor enters into the heterogeneous water-shed, to integrate, in effect, all of these values. This has been done with success, to the writer's knowledge, to the point that the correct volume or total run-off has been determined closely. The carrying of this flow through the tortuous ways

[15] "Modifications of the Index-Area Principle and the Anticipated Application of the Principle to Muskingum River Flood Control," by Waldo E. Smith, *Transactions*, Am. Geophysical Union, Part I, 1938, p. 455.

[16] "Synthetic Unit Graphs," by Franklin F. Snyder, Jun. Am. Soc. C. E., *loc. cit.*, 1938, p. 447.

[17] "The Structure of Discharge-Recession Curves," by B. S. Barnes, Assoc. M. Am. Soc. C. E., *loc. cit.*, Part IV, 1939, p. 721; also, "A Conception of Run-Off Phenomena," by Franklin F. Snyder, Jun. Am. Soc. C. E., *loc. cit.*, p. 725.

[18] "Run-Off—Rational Run-Off Formulas," by R. L. Gregory and C. E. Arnold, *Transactions*, Am. Soc. C. E., Vol. 96 (1932), p. 1038.

[19] "Relation Between Rainfall and Run-Off from Small Urban Areas," by W. W. Horner and F. L. Flynt, *loc. cit.*, Vol. 101 (1936), p. 140.

of overland and subsurface flow, and channel and valley storage, to the gaging station is similarly under study with results, so far as the writer is apprised, as yet inconclusive. However, the number of variables involved and the evaluation of these variables even on a small water-shed suggest the difficulty in (and even the uncertainty of) the ultimate solution of the problems involved as suggested by the author. The distribution-graph method, as such, is probably not sufficiently refined to be of service in connection with such a solution if and when it is found. In the meantime, it serves as a very handy tool.

AMERICAN SOCIETY OF CIVIL ENGINEERS

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DISCUSSIONS

FUNCTIONAL DESIGN OF FLOOD CONTROL RESERVOIRS

Discussion

By Messrs. Sherman M. Woodward, M. Kindinger, P. Wilhelm Werner, and Waldo E. Smith

Sherman M. Woodward,[17] M. Am. Soc. C. E. (by letter).[17a]—An enlightening, interesting, and useful extension of the methods used about 1915 in the design of the reservoirs of the Miami Conservancy District at Dayton, Ohio, is presented in this paper. At that time the so-called "five-sixths rule" was finally developed by the engineering staff of the District after a prolonged period of studying and puzzling over the various complex relationships involved in the determination of the required capacity of the several reservoirs and the dimensions of their respective outlets. It was recognized that no great virtue or precision should be attached to the exact numerical value, five-sixths, and that this coefficient should be expected to change with circumstances and should be checked in each important case by further detailed calculations.

The practical value of the "five-sixths rule" is that for the purpose of approximate estimates and comparisons in preliminary design, before dimensions are finally fixed, it gives results in a few minutes which would otherwise be obtained only after days of laborious computation. The rule was evolved as a result of a determined effort to discover a general, broad-visioned, practical method of approach to a complicated problem, an approach which should give due consideration to the main, essential factors and at the same time minimize the unimportant, non-essential factors.

The authors have performed a valuable service in throwing new light upon some of the relationships. The writer believes that the possibilities of the subject have not yet been exhausted and hopes that one effect of the paper will be to reveal new directions for further profitable exploration.

One item of particular importance in the paper is the direct comparison that can now be made between the relative advantages of the use of automatic versus manual-controlled reservoir outlets.

Note.—This paper by C. J. Posey, Jun. Am. Soc. C. E., and Fu-Te I, Esq., was published in October, 1939, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: December, 1939, by Edward Soucek, Jun. Am. Soc. C. E.; and March, 1940, by Messrs. J. C. Stevens, Edward J. Bednarski, Ronald A. Kampmeier, and Edgar E. Foster.

[17] Chf. Water Control Planning Engr., TVA, Knoxville, Tenn.

[17a] Received by the Secretary February 9, 1940.

M. KINDINGER,[18] Assoc. M. Am. Soc. C. E. (by letter).[18a]—Table 2 can be condensed to a single set of data by tabulating d against $\frac{n}{m}$ instead of m. Its use will then be extended to a wide range of outlet types besides those of the orifice and weir.

In Equation (5), substituting the value of h determined from Equation (1), and considering o and s to be maxima,

$$O = \frac{B\, s^u}{C^u} \qquad (34)$$

in which (to simplify typography), $u = \frac{n}{m}$. An inspection of Table 2 will reveal that for the same values of u and x the detention ratio is identical in both sets, as demonstrated for a few values of u in Table 6.

TABLE 6.—VALUES OF m AND d COMPARED WITH TABLE 2

$u = \frac{n}{m}$	m		d			
	n = 0.5	n = 1.5	0.2	0.4	0.6	0.8
0	...	...	0.800	0.600	0.400	0.200
0.1	5	15	0.818	0.635	0.450	0.258
0.2	2.5	7.5	0.832	0.662	0.488	0.303
0.5	1	3	0.864	0.722	0.570	0.399
1.0	0.5	1.5	0.896	0.789	0.656	0.498
1.5	0.33	1.0	0.916	0.822	0.710	0.567
2.0	0.25	0.75	0.939	0.849	0.749	0.619

The formulas given in the Appendix can be converted by substituting u for $\frac{n}{m}$. For example, Equation (19) then becomes:

$$r = \frac{1}{1+u}\left[1 + \frac{u^2\, x}{(1+u)\,(1+2\,u)} + \frac{(1+u+2\,u^2)\,u^2\,x^2}{(1+u)^2\,(1+2\,u)\,(1+3\,u)} + \cdots\right] \qquad (35)$$

The statement that the equivalent uniform flood can be selected readily on an arbitrary basis is open to question. The close agreement shown in Table 1 is not a convincing demonstration because it is always possible to compute the exact equivalent uniform flood which, for any flood volume one wishes to choose, will give the same storage and peak outflow as the original: $x = \frac{O}{I}$; and (if V is the flood volume) $I = \frac{V}{T}$. Therefore,

$$T = \frac{x\,V}{O} \qquad (36)$$

[18] Roads Engr., City Council of Pretoria, Pretoria, Union of South Africa.

[18a] Received by the Secretary January 24, 1940.

In the authors' example $d = \frac{117,100}{292,800} = 0.40$; but from Table 2(*a*), $x = 0.697$ and, therefore, $T = \frac{0.697}{5,160} \times 292,800 = 39.4$ hr. The problem most common in practice, however, is the reverse one. The engineer must guess, or otherwise determine, T first, and then use its value to compute the value of d required to reduce the peak outflow to a particular magnitude.

It is clear that with outflow peaks near to, or greater than, 7,320 acre-ft per hr the 40-hr equivalent will give unreliable results. Evidently, therefore, T must be so chosen as to give an equivalent flood intensity I greater than the peak outflow; and hence, T will vary from case to case.

Furthermore, the peak outflow is not influenced in any way by that part of the flood curve that lies beyond the point of equalization (the point where outflow equals flood inflow and where, also, the outflow is at its peak). Hence, no apparent purpose is served by the condition that the equivalent uniform flood shall have a volume equal to that of the entire flood. Rather, it would appear to be advantageous if the volume of the equivalent were made equal to that of the inflow or flood at the point of equalization.

In studying flood routing problems the writer has found that the mass curve is far more serviceable than the intensity curve.

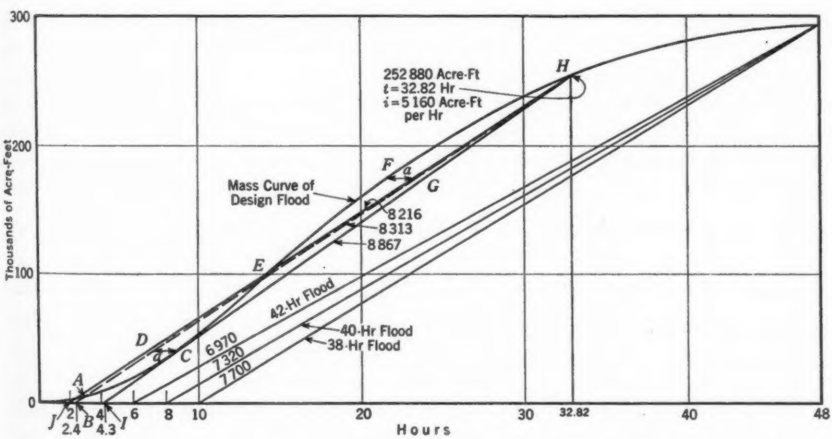

FIG. 5.—MASS CURVES OF VARIOUS SUBSTITUTE UNIFORM FLOODS COMPARED WITH MASS CURVE OF DESIGN FLOOD

In Fig. 5, which is a plot of the authors' example, H is the point of equalization. The storage volume will depend upon the shape and dimensions of the part of the curve $O\ C\ E\ F\ H$. Obviously, any equivalent uniform flood should deviate from this curve as little as possible. Starting from the known value of S and computing T, omitting the volume of inflow after the point of equalization, the period of the exact equivalent is found to be 30.8 hr. It is shown in Fig. 5 by the full line $J\ H$.

The following rule enables a substitute uniform flood curve to be drawn which will fit the original closely and yet give a value of the storage S which errs on the side of safety:

> Draw the chord $B\,H$, shown dotted, in such a manner that both the length of intercept, $H\,E$, and the horizontal divergence, $F\,G$, of the convex part are respectively equal to, or larger than, the corresponding intercept, $E\,A$, and divergence, $D\,C$, of the concave part.

It can be proved, or verified by means of any of the step methods, that:

(*a*) A part of a mass curve concave upward gives a lesser flood-reducing effect than the chord joining its extremities and that the reverse is the case for a convex curve (it will be realized that the greater the increment in storage required to deal with the increment of the flood volume represented by the part of the flood curve in question the smaller will be the flood-reducing effect); and

(*b*) This difference in flood-reducing effect between chord and curve increases with length of chord, amount of divergence, and rate of outflow.

Even if the concave part, $A\,C\,E$ (except for its reversed position), were identical in shape and dimension with the convex part, $E\,F\,H$, the larger outflow prevailing during the latter (convex) period would result in a greater difference in flood-reducing effect between chord and curve than would obtain during the earlier concave period. In addition, the flatter slope of the part, $O\,A$, in the beginning is superior in flood reduction to the chord $B\,A$. Hence, taking the substituted chord as a whole, it will have a lower flood-reducing effect than the original. In other words, it requires a higher storage S to reduce the peak to O. Another substitute that is easier to draw but has the disadvantage of giving a still more conservative result is the chord $I\,H$ (Fig. 5), which is tangent to the concave part. For the purpose of comparison, computations have been made for $\frac{n}{m} = 2.0$, which gives a value of u at the opposite end of the field to that covered by the authors' $\frac{n}{m} = \frac{0.5}{2.5} = 0.2$. Storage S was determined graphically for the original flood, and by computation for the various substitute uniform floods. The results are summarized in Table 7.

TABLE 7.—COMPARISON OF MAXIMUM STORAGE FOR VARIOUS SUBSTITUTE UNIFORM FLOODS

Line	Type (see Fig. 5)	I	O	V	x	$u = 0.2$		$u = 2.0$	
						d	S	d	S
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1	Original	12,100	5,160	252,880	0.426	0.463	117,100	0.708	179,000
2	$J\,H$	8,216	5,160	252,880	0.628	0.463	117,100	0.733	185,300
3	$B\,H$	8,313	5,160	252,880	0.621	0.469	118,600	0.737	186,400
4	$I\,H$	8,867	5,160	252,880	0.582	0.504	127,440	0.759	191,900
5	44-hr	6,688	5,160	292,800	0.772	0.329	96,400	0.639	187,100
6	40-hr	7.320	5,160	292,800	0.705	0.392	114,800	0.688	201,500
7	36-hr	8,150	5,160	292,800	0.635	0.456	133,500	0.729	213,500

Arbitrary selection of the equivalent flood can lead to great uncertainty. Thus, in line 5, Column (7), Table 7, a 10% variation in T results in a negative error of 17% in S, whereas line 7, Column (9), shows that a similar variation in the opposite direction involves a result 19% too high. The substitute uniform flood drawn according to the foregoing rule shows positive errors of less than 5%.

P. WILHELM WERNER,[19] Assoc. M. Am. Soc. C. E. (by letter).[19a]—This interesting study is valuable for the investigation of an important type of storage problem. It must be borne in mind, however, that it is strictly applicable only under certain conditions. For instance, if the reservoir outlet consists of a tunnel of some length, the discharge rating curve cannot be expressed, even approximately, by a monomial exponential function such as Equation (5). In the case of a pure orifice outlet the tailwater may sometimes influence the discharge so as to cause a considerable deviation of the rating curve from the monomial exponential function.

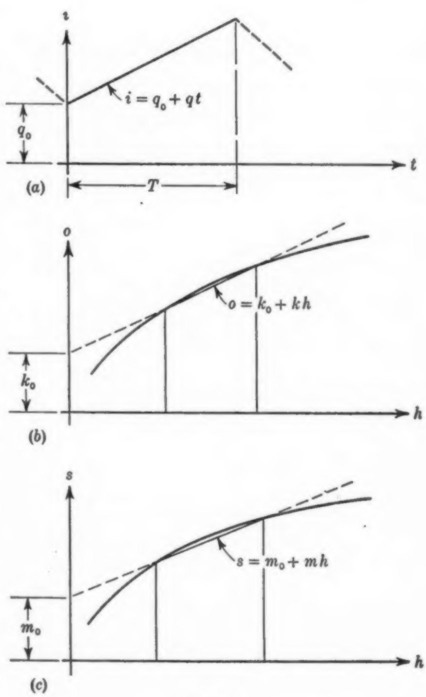

FIG. 6

As stated by the authors, flood routing studies are commonly executed by the trial-and-error step method or the integration method. In many cases, however, a combination of the two methods is useful. One such combination (which might be called the "straight-lines integration method") may be of interest in this connection. In its application this method is of the functional type, the integration, however, being executed in steps over a number of straight lines covering, as nearly as practicable, the given characteristic curves. Using the same nomenclature as the author, as far as possible, the procedure may be demonstrated as follows.

The inflow may be written

$$i = q_o + q\,t \qquad (37)$$

in which q_o is the intercept on the i-axis and q the slope of the straight line representing the inflow intensity during the interval T (see Fig. 6(a)). The outflow may be written

$$o = k_o + k\,h \qquad (38)$$

[19] Civ. Engr., A. B. Vattenbyggnadsbyran, Stockholm, Sweden.

[19a] Received by the Secretary February 20, 1940.

in which k_o is the o-intercept and k the slope of the straight line, substituting the outflow intensity curve in the part under consideration (see Fig. 6(b)). The depth-capacity may be written

$$s = m_o + m\,h \quad \text{.......................} \quad (39)$$

in which m_o is the s-intercept and m the slope of the straight line to be substituted for the considered part of the depth-capacity curve (see Fig. 6(c)).

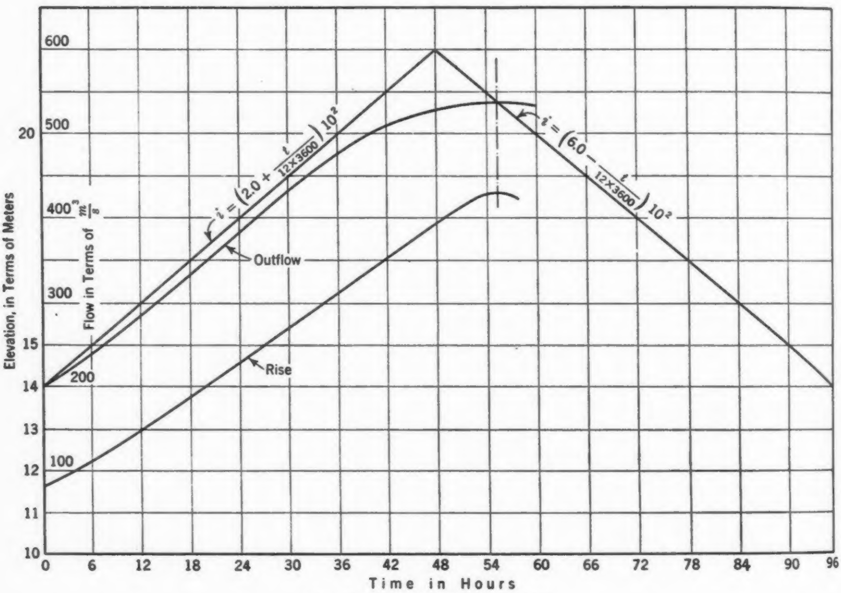

Fig. 7.—Inflow Hydrograph

Substituting Equations (37), (38), and (39) in the general differential equation (Equation (4)), and reducing:

$$m\frac{dh}{dt} + k\,h = q\,t - (k_o - q_o) \quad \text{................} \quad (40)$$

Of the constants, q_o, m, and k are always positive, whereas q and k_o may be either positive or negative. If $t = o$ and $h = h_o$ are corresponding values, after integrating Equation (40), and reducing:

$$h - h_o = -(B + h_o)(1 - e^{-\alpha}) + A\,t \quad \text{.............} \quad (41)$$

in which (to simplify typography) $\alpha = -\dfrac{k}{m}\,t$; and

$$A = \frac{q}{k} \quad \text{........................} \quad (42a)$$

and

$$B = \frac{1}{k}\,(k_o - q_o + m\,A) \qquad \text{(42b)}$$

If a theoretical maximum occurs in some interval, it is determined thus:

$$\frac{dh}{dt} = -\,(B + h_o)\,\frac{k}{m}\,e^{-\alpha} + A = 0 \qquad \text{(43)}$$

or, after reducing,

$$t = t_m = \frac{m}{k} \times l\,n\left(\frac{B + h_o}{A}\,\frac{k}{m}\right) T \qquad \text{(44)}$$

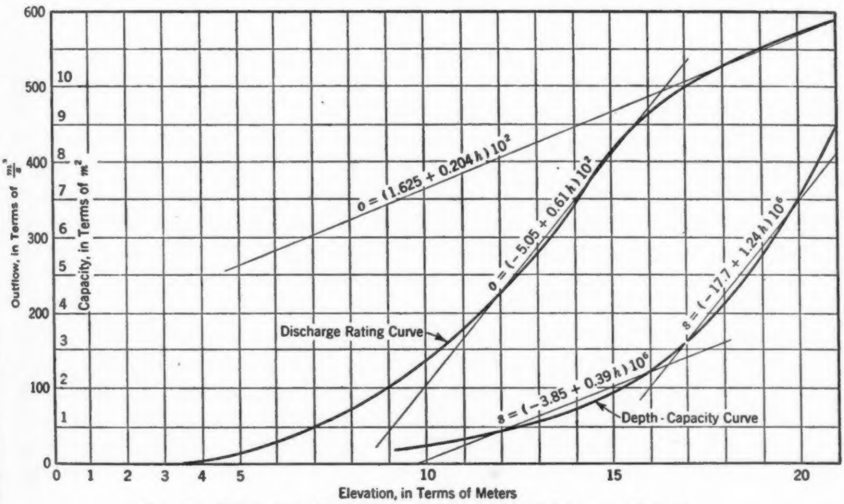

FIG. 8.—DISCHARGE RATING CURVE AND DEPTH-CAPACITY CURVE

The application of the foregoing deductions may be illustrated by the following example: The rise of the water level in a reservoir is to be determined for the flood given by the inflow hydrograph in Fig. 7. The water discharges through a by-pass tunnel, the discharge rating curve of which is shown in Fig. 8. The depth-capacity curve is also given in Fig. 8.

TABLE 8.—COMPUTATION OF RISE IN A RESERVOIR

h_o	q_o	q	k_o	k	m	A	B	$B + h_o$	$h - h_o$	$\frac{t}{3{,}600}$	h
11.60	200	$\frac{1}{432}$	−505.0	61.0	390,000	$\frac{1}{26{,}300}$	−11.3	+ 0.3	4.80	37.3	16.40
16.40	511	$\frac{1}{432}$	+162.5	20.4	1,240,000	$\frac{1}{8{,}820}$	−10.15	6.25	1.45	10.7	17.85
17.85	600	$-\frac{1}{432}$	+162.5	20.4	1,240,000	$-\frac{1}{8{,}820}$	−28.35	−10.5	0.70	7.1*	18.55

* This value computed by means of Equation (44).

The computations, in three main steps, are summarized in Table 8, and the result is shown graphically in Fig. 7. A check by means of the usual trial-and-error step method shows a close agreement, the discrepancy in the total rise amounting to about 2%.

In comparison with the functional method developed by the authors, the foregoing suggestions possess the advantage of being applicable with sufficient accuracy to any storage problem of the nature in question, including all types of reservoirs and outlets. It also avoids the uncertainty in estimating the equivalent uniform flood. Compared with the trial-and-error step method it effects a saving in time and labor, inasmuch as the tedious cut-and-try operations have been eliminated. For office use a graphical evaluation of Equation (41), in some form or other, is often very helpful.

WALDO E. SMITH,[20] M. AM. SOC. C. E. (by letter).[20a]—An interesting contribution to the study of the functional design of reservoirs for flood control is found in this paper. Often the time-saving features of this method of establishing tentative basic values for a reservoir for preliminary estimates and as a guide in design have been overlooked, and it is hoped that this reminder of earlier studies will serve to call attention to this method of approach. However, the construction of gate controls on outlet works is the current tendency for flood control dams, and with this type of outlet, functional analysis loses much of its significance. It may still serve, none-the-less, on preliminary studies to give the value of the minimum allowable size of outlet.

The authors have failed to recognize the most common justification for the use of gate or mechanically controlled outlets. They assume that such outlets are for the purpose of maintaining 100% "efficiency," meaning, presumably, that outflow is uniform from the beginning to the end of the flood. The writer believes that this is incorrect usage of the term "efficiency" unless the only area one is attempting to protect from floods is that immediately below the dam. Ordinarily, one must relieve a prospective flood situation at some point or points considerably downstream, and must vary the outflow to accommodate the uncontrolled inflow into the channel at points between the control works and the points being protected. It is to provide for this needed flexibility that gate controls are ordinarily used. To quote from the Official Plan for the Muskingum Watershed Conservancy District:[21]

> "*Reasons for Selection of Gate Controlled Reservoirs.*—Careful study of the tributary arrangement of the Muskingum River and the time of travel of past flood crests on the tributaries show that the use of retarding basins will not provide either as efficient or as economical flood control as will gate controlled reservoirs. The principal objection to the retarding basin scheme as applied to the tributaries of the Muskingum River is based on the design principle of the system, that is, that the maximum outflow from any reservoir during the design flood must not, when combined with the outflow from other reservoirs and/or flows from uncontrolled areas, cause flows in excess of those for which provision has been made at any point downstream. * * * With gate control, the outflow from the reser-

[20] New Philadelphia, Ohio.

[20a] Received by the Secretary March 4, 1940.

[21] "Official Plan for the Muskingum Watershed Conservancy District," Vol. I, pp. 23 and 24.

voirs can be reduced during high discharge from uncontrolled areas and increased after the rate of runoff below the reservoirs has receded."

In discussing the depth-capacity exponent, m, the authors state that depths above the outlet location are used, whether the outlet is an orifice or a weir. It has been the writer's procedure to base this value, and the resulting exponential equation, on the height above approximate zero reservoir capacity, as explained by Professor Woodward for the case of variable depth.[22] In all instances that the writer can recall, it has been possible to make nearly all of the points fall very close to a straight line on logarithmic paper. The resulting equation in the form:

$$s = C\, h^m \qquad (45)$$

has the advantage of defining the storage accurately throughout the depth of the reservoir. For general purposes, the method suggested by the authors has two undesirable results: First, it has the tendency to change the natural valley characteristics as indicated by m and C; and second, it frequently results in a plotting of points that does not approach a straight line, with the result that no satisfactory equation can be written.

Pleasant Hill Reservoir in Ohio is a case in point. There a weir-orifice outlet will maintain the water surface at an elevation of about 1,020 ft, nearly 50 ft above the channel bottom. Fig. 9 shows the depth-capacity curves based on the value of approximate natural zero capacity and based on Elevation 1,020. The uppermost plotting was the first attempt, based on an estimated zero capacity at Elevation 968. A second attempt, not shown, was made before arriving at the second line from the top. This straight line passes very close to all points, with zero capacity at Elevation 973. The formula for this curve is expressed by Equation (45), with $C = 0.29$ and $m = 2.80$. It serves capacity needs throughout the entire depth of the reservoir as well as of any part in which one might be especially interested, and m and C serve as correct indications of morphological conditions within the valley, as mentioned by the authors.

Using only that portion of the reservoir capacity above Elevation 1,020 and measuring depth from that elevation, a third plotting of points is shown. These do not follow a straight line. If one uses the equation of the tangent at the upper end of the curve defined by the points, $C = 180$ and $m = 1.58$, values of capacity determined in the vicinity of the highest elevation will be quite satisfactory; but nearer to the weir-orifice level, the values will serve no purpose. If one derives the equation of the secant, drawn through the uppermost and lowermost points shown—$C = 590$ and $m = 1.29$—in the vicinities of these two points values of total storage may be approximated, but intermediate values and differentials will have little significance. As demonstrated subsequently, the value of m for the entire reservoir gives a better value of maximum outflow, in this case, than either of the other two values determined. It should be noted that the characteristics of the valley apparently change

[22] "Hydraulics of the Miami Conservancy District," by Sherman M. Woodward, Technical Reports of the Miami Conservancy District, Part VII, pp. 118–121.

markedly, m changing from 2.80 (hill type) to 1.58 (flood-plain foothill type) to 1.29 (lake type). Possibly the latter value has some use in expressing the nature of the valley between two given horizontal planes; but by taking other than the natural zero of storage capacity, the measure of natural valley char-

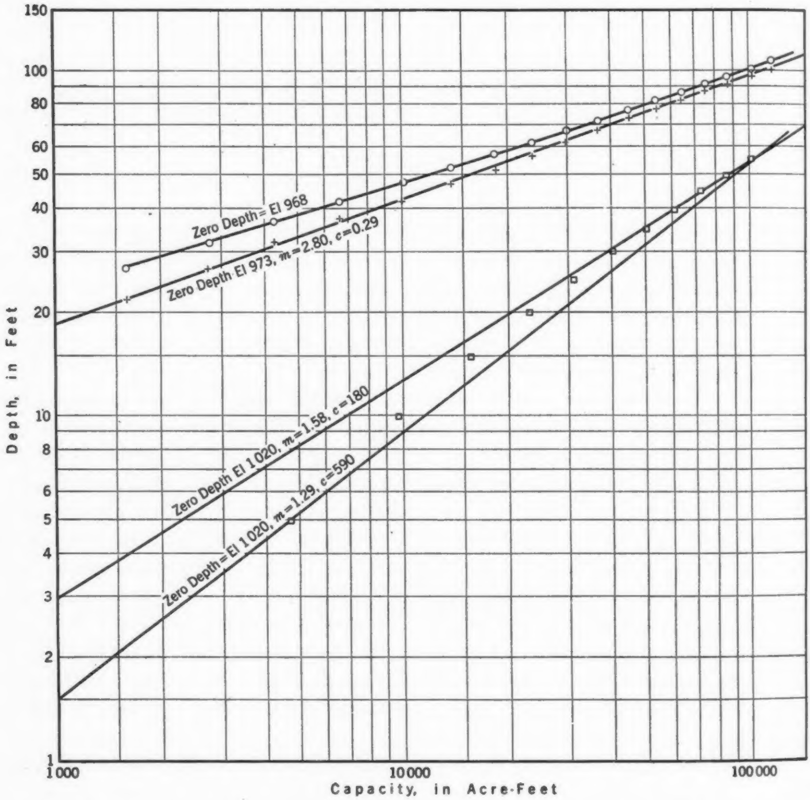

FIG. 9.—DEPTH CAPACITY CURVES; PLEASANT HILL RESERVOIR, MUSKINGUM VALLEY, OHIO

TABLE 9.—CONSTANTS IN EQUATION (45); SENECAVILLE RESERVOIR, OHIO

Elevation of zero depth	Description	Constants: C	Constants: m
805	Natural	30	2.20
812	Sill of outlet gates	188	1.79
831	Crest of conservation weir	2,430	1.24

acteristics expressed in m and C is destroyed. That this acts likewise for other reservoirs is shown by reference to Table 9 for Senecaville Reservoir in Ohio.

In Table 2, it should be noted that d varies relatively slightly within the entire range of m, as compared with the variation within the range of x. Thus factors entering into the variation of x are likely to be of greater importance that those entering into m. One of these is the value of the inflow rate of the uniform equivalent flood, which, in turn, is dependent on the duration of that flood and its nature otherwise.

The authors have not discussed, in satisfactory detail, the determination of the equivalent uniform flood. If one is entirely arbitrary in the selection of the duration of the equivalent uniform flood, it is likely to result in gross error. The duration should be approximately that period from the beginning of the natural flood to the time following the crest at which inflow and outflow are equal but, as demonstrated subsequently, this seems not to give satisfactory results with the method outlined by the authors. Using first, as an example, the authors' triangular flood hydrograph, with a 40-hr duration of uniform equivalent flood, x is found to be 0.702. If, on the other hand, 30 hr had been selected, the uniform rate would have become 9,760 acre-ft per hr and x would have become 0.537 with a corresponding d from Table 2 equal to 0.543. Then the total storage required would become 159,000 acre-ft instead of 117,100 acre-ft—a very substantial difference in result.

Citing the Pleasant Hill Reservoir in Ohio as a further example, in this case, the design flood was a 9-in. runoff from a 5-day rain. The storage was previously established as 7 in. of runoff from the 199-sq-mile drainage area above. Thus the detention ratio, d, is known to be 0.778, and low outflow, amounting to only 2 in. during the flood period, is known. From hydrographs of other simple floods of comparable duration, the time from the beginning of the flood to the time that inflow and outflow would become about equal after passage of crest inflow was known to be about 5.5 days. The resulting equivalent uniform flood is equal to 1.64 in. per day, or 8,780 cu ft per sec. Using the secant value of m equal to 1.29, Table 2 gives x to be equal to 0.303, and the maximum outflow is found to be 2,670 cu ft per sec. With the step method of routing, the correct value was found to be 2,350 cu ft per sec, not a very satisfactory check. Using the tangent equation, the value becomes 2,530 cu ft per sec. Using the equation for the entire reservoir, it becomes 2,280 cu ft per sec, the best of the three by this method.

Assuming the five-sixths rule to hold, however, one can establish the approximate maximum rate of outflow directly, knowing its volume to be 2 in. from the watershed above, over a period of 5.5 days. The average, which is five sixths of the maximum, is 1,950 cu ft per sec. The maximum, in turn, is found to be 2,340 cu ft per sec, a very good check. It should be noted that this does not require the use of any uncertain values of m or x.

The authors suggest the term "outflow ratio" for the ratio between maximum outflow rate and the inflow rate for the equivalent uniform flood, which is also designated as x. This is an excellent suggestion, but the writer would suggest that it be used also with respect to the ratio between the maximum outflow rate and the maximum inflow rate of the natural flood from which the equivalent uniform flood is derived They are in error, however, in stating

that this term designates the same value as Professor Woodward's "protection ratio." At no point has the writer been able to find that Professor Woodward gave any definition for x other than $\frac{0}{I}$.[23] "Protection ratio," as used by him without definition,[24] is a satisfactory expression, and is the peak inflow divided by the peak outflow. It is a ratio always equal to, or greater than, 1, and the reciprocal of outflow ratio if the authors will accept the writer's modification of their term "outflow ratio."

[23] "Hydraulics of the Miami Conservancy District," by Sherman M. Woodward, Technical Report of the Miami Conservancy District, Part VII, p. 186.

[24] *Loc. cit.*, p. 197.

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DISCUSSIONS

GENERAL WEDGE THEORY OF EARTH PRESSURE

Discussion

By M. G. Spangler, Assoc. M. Am. Soc. C. E.

M. G. Spangler,[24] Assoc. M. Am. Soc. C. E. (by letter).[24a]—The introduction of a logarithmic spiral to define the surface of rupture, instead of the usual straight line, in computations of the lateral earth pressure on a restraining structure by the Coulomb type of theory, is an interesting and ingenious procedure. There is ample justification for assuming the surface of rupture to be similar in shape to this equiangular spiral, since many observations of cracks in the earth behind restraining structures have indicated the essentially vertical orientation of the ruptured surface for considerable depths below the natural ground level and, although actual observations of the lower extremities of the path followed by this surface are nonexistent, there is no reason to doubt that it bends toward and passes through the base of the structure. The author has made a welcome contribution by showing that even when the surface of rupture deviates from a plane as much as does the logarithmic spiral surface, the error introduced by using the plane of Coulomb's theory does not exceed 6%. This is a comforting revelation.

The dearth of actual measurements of both the distribution and magnitude of lateral earth pressures on the timbering of excavations adds greatly to the interest in that part of the paper which deals with the field measurements of pressures in the struts of a deep subway cut in Berlin. Such measurements, when properly interpreted, are of the utmost value and there is need for many more case studies of this kind.

In the case reported, however, there is a real question as to whether the pressure magnitudes and distributions shown in Fig. 9 are justified by the data obtained in the measuring operations. The steel I-beams which were driven along the outside faces of the cut, and against which the sheathing planks were placed, are shown in Fig. 4 to project downward into the sand and gravel ma-

Note.—This paper by Karl Terzaghi, M. Am. Soc. C. E., was published in October, 1939, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: January, 1940, by Howard F. Peckworth, M. Am. Soc. C. E.; and February, 1940, by Jacob Feld, M. Am. Soc. C. E.

[24] Research Associate Prof., Civ. Eng., Iowa Eng. Experiment Station, Iowa State Coll., Ames, Iowa.

[24a] Received by the Secretary February 26, 1940.

terial an appreciable distance below the bottom of the cut. If this illustration is correct, the lower end of these beams must have exerted a sizable horizontal reaction against the sand and gravel below the bottom of the cut, and the sum of the compressions in the four horizontal struts probably represented only a portion of the total lateral pressure acting against the sheathing. These vertical beams, instead of acting as a continuous beam over four supports, as considered by the author, appear in the diagram to have been continuous over five supports, and since sand and fine gravel would provide a relatively rigid bearing support, it seems likely that this fifth support may have carried an appreciable percentage of the total lateral load. Therefore, the pressure magnitudes and the high center of gravity of the resultant pressures, as shown in Fig. 9, remain uncertain until more definite knowledge of the installation conditions is available.

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DISCUSSIONS

RELATION OF THE STATISTICAL THEORY OF TURBULENCE TO HYDRAULICS

Discussion

BY MESSRS. CLYDE W. HUBBARD, JOHN S. MCNOWN, AND SAMUEL SHULITS

CLYDE W. HUBBARD,[36] ESQ. (by letter).[36a]—Compliments are due the author on his clear and simple exposition of the theory of turbulence. More and more it is necessary for the hydraulic engineer to study the mechanism of turbulence in the hope that some of the unexplained phenomena of flowing water will become clear.

However, the author states in the conclusion of his paper that: "The static pressure read by a static-pressure tube placed in a turbulent fluid has a reading greater than the true static pressure by an amount proportional to the mean squares of the cross-velocity components." If this were true, the pitot differential would be less than the true velocity head and the coefficient of the pitot tube would be larger than unity. This is contrary to the experience of practically all investigators who have made tests with ordinary types of pitot tubes in pipes.

The author also states that the error of a well-designed pitot tube or static pressure tube should be less than 1%. In other words, the customary form of pitot tube can be relied upon to read the true velocity within 1% in the turbulent flow normally found in pipes.

Tests described by the writer[14] and by E. Shaw Cole,[37] Jun. Am. Soc. C. E., in 1939, show that for the ordinary form of pitot tube the error caused by turbulence may be as much as 5% and ordinarily is about 2%. In connection with these tests the intensity of turbulence or "mean angularity" of flow was measured in pipes from 12 in. to 72 in. in diameter at velocities ranging from 3 to 8 ft per sec.

NOTE.—This paper by A. A. Kalinske, Esq., was published in October, 1939, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: January, 1940, by Hunter Rouse, Assoc. M. Am. Soc. C. E.; February, 1940, by Messrs. Martin A. Mason, and J. C. Stevens; and March, 1940, by Boris A. Bakhmeteff, M. Am. Soc. C. E.

[36] Asst. Prof., Hydr. Eng., Worcester Polytechnic Inst., Worcester, Mass.

[36a] Received by the Secretary February 16, 1940.

[14] "Investigation of Errors in Pitot Tubes," by C. W. Hubbard, *Transactions*, A. S. M. E., Vol. 61, 1939, p. 477.

[37] *Loc. cit.*, p. 495.

No measurable error in the reading of the impact orifice of a pitot tube was found to be caused by turbulence, which is in agreement with the author's statement. However, turbulence did appear to cause an error in the reading of the pressure head by either a wall piezometer or by a static orifice on the smooth tip of a pitot-static tube. This error in the pressure reading, although important in pitot-tube measurements, was not great enough to affect, seriously, the accuracy of pressure head measurements for pump and turbine efficiency tests.

Mr. Cole[37] describes tests with various simple pitot tubes using carefully constructed wall piezometers made in three 12-in. pipes having different flow patterns. The coefficients obtained for pitot tubes were different in each of the three pipes. Since the impact orifice was found to have no appreciable error, it can only be inferred that the change in coefficient was caused by errors in the wall piezometer readings.

The wall-piezometer error varied with the ratio of mean to center velocity, known as the pipe factor. The error increased proportionately as the pipe factor decreased below unity, so that a coefficient of 0.968 was obtained in the roughest pipe having a pipe factor of 0.77.

The writer described similar tests with a smooth pitot-static tube at the center of the same three pipes in the closure to his paper.[38] The same pitot tube was tested at the center of each of the pipes and the coefficients obtained were 0.992, 0.980, and 0.956. These coefficients, however, did not bear a simple relation with the pipe factor as did Mr. Cole's tests with wall piezometers; nor did the coefficient appear to depend on the intensity of turbulence alone.

It is possible that, when the scale (mean eddy size) as well as the intensity of turbulence are both measured successfully, some rational explanation of the error of the pitot-static orifice will be forthcoming. Near the wall of the pipe both the scale and intensity of turbulence are dependent on the condition of the pipe surface, and any turbulence caused by an upstream disturbance would be dissipated rapidly in the small intense eddies. It would be expected, therefore, that an error of the wall piezometer caused by turbulence would vary with the wall roughness and hence the pipe factor. On the other hand, the turbulence at the center of the pipe is largely of the isotropic type which does not produce shearing stress in the fluid. The scale and intensity of turbulence which exists may be dependent on upstream conditions, since the eddies are larger and the decay less rapid. Therefore, the error of a pitot-static pressure orifice at the center of a pipe, caused by turbulence, would not necessarily bear a consistent relation to the pipe factor or to the intensity of turbulence alone, but may be dependent partly on the scale of turbulence as yet not measured in these pipes.

One method of avoiding the effect of the existing turbulence in the center of a pipe on the pressure orifice of a pitot-static tube is to create a disturbance of the flow immediately upstream from it. A small ring was placed on the smooth tip just ahead of the pressure orifice and the tests in the three 12-in. pipes repeated. The coefficients obtained were 0.897, 0.885, and 0.889 as com-

[38] "Investigation of Errors in Pitot Tubes," by C. W. Hubbard, *Transactions*, A. S. M. E., Vol. 61, 1939, p. 504.

pared with 0.992, 0.980, and 0.956 with the smooth tip. The ring improved the consistency considerably, although the coefficient was decreased.

The test results described herein show that turbulence does appreciably affect the accuracy of pitot-tube measurements and causes the coefficient to be less than unity. However, the pitot tube remains an accurate instrument for flow measurement when properly used by experienced engineers. Errors caused by turbulence can be avoided by the use of a type of tube that is but slightly affected and by calibrating it under flow conditions similar to those under which it is to be used.

JOHN S. MCNOWN,[39] JUN. AM. SOC. C. E. (by letter).[39a]—Professor Kalinske has presented several excellent methods of approach to the difficult problem of turbulence, and has substantiated many of his conclusions by laboratory measurements. With so many phases of hydraulics depending on turbulence, the need for study and laboratory research on turbulence in hydraulics has become more and more pressing. This paper gives the fundamental principles, points out the fields of application, and describes the laboratory approach in detail. In addition, an important function fulfilled by this paper is the bridging of the gap between laboratory data and the mathematical theories of statistics and turbulence.

The photographic technique outlined by the author is an improvement over other existing methods for measuring instantaneous velocities in water. Any type of apparatus that must be placed in the water is indeed of doubtful accuracy until proved otherwise. One apparent difficulty with the photographic measurements, in addition to that of analyzing the pictures, is indicated by the angular character of the curve in Fig. 1 which gives the relationship between time and instantaneous velocity fluctuations. An increase in the number of pictures taken per second should smooth out the curve, but would increase the difficulty in analyzing the individual frames.

Several questions arise from the discussion of the quantity λ, defined, following Equation (11), by the formula

$$\lambda = \underset{x \to 0}{\text{Limit}} \left(\frac{x}{\sqrt{1 - R_x}} \right) \dots\dots\dots\dots\dots\dots (30)$$

and its use in the determination of the energy loss resulting from turbulence. The author does not give the conditions under which the data used for this analysis were obtained or the degree to which the isotropy assumed in the development was attained. Furthermore, the method of determining the numerical value of λ is not fully understood by the writer. A knowledge of the functional relationship between x and R_x would be necessary to determine the value of the limit given in Equation (30) as the definition of λ. In a treatment of the same topic in a report by the Fluid Motion Panel of the Aeronautical Research Committee[40] a knowledge of the value of the quantity $\overline{\left(\frac{\partial v}{\partial x}\right)^2}$ is used

[39] Instr. of Math., Mechanics and Hydraulics, Univ. of Minnesota, Minneapolis, Minn.

[39a] Received by the Secretary February 19, 1940.

[40] "Modern Developments in Fluid Dynamics," by the Fluid Motion Panel of the Aeronautical Research Committee and edited by S. Goldstein, pp. 223–225.

in computing the value of λ, whereas the author computes λ independently and uses it to compute $\overline{\left(\frac{\partial v}{\partial x}\right)^2}$. The assumption of a parabolic distribution near the peak of the curve, as proposed by G. I. Taylor,[41] would make the reverse order of computation possible. The validity of such assumption depends upon the agreement between the curves at the peak.

The several lengths defined as helpful concepts in the study of turbulent flow must be interrelated to some extent. The concept of the correlation of instantaneous velocities at two points in a stream appears to be closely related to one of Prandtl's two definitions of a mixing length—namely, the diameter of the mass of fluid moving as a whole.[42] A comparison of Prandtl's mixing length, the length λ, the length represented by the x-intercept of the curve in Fig. 3 ($x = 4.6$ in., approximately), or the length criterion defined by Taylor would be interesting if possible. The aforementioned curve in Fig. 3 gives the relationship between x, the distance between two points, and R_x, the correlation between the instantaneous velocities at the two points. The x-intercept of this curve would be the least distance at which no correlation exists.

Among the hydraulic problems affected by turbulence is that of bed-load movement. One troublesome feature of bed-load studies is the lack of unity in interpreting the meaning of "beginning movement," and the application of this concept to other stages. Usually experimenters refer their results to this stage as a basis for computation in using such quantities as excess bottom velocity and effective tractive force. Conceivably part of the difficulty in reaching a common interpretation of this reference stage and the resulting relationships is caused by failing to consider the variable of degree of turbulence. A high degree of turbulence would cause an initial movement at comparatively low values of tractive force as the local velocities are of greater magnitude than for less turbulent streams. Furthermore, this difference becomes less with increasing velocity as the intermediate local velocities become more important. Thus, the effect of turbulence should be investigated in standardizing the results of comparable research problems. A difficulty in such an analysis is the probable absence of isotropy in the turbulence near the boundary where its effect is most important in this problem.

The use of the letter Q to represent weight of water flowing per unit of time is a departure from general hydraulic practice. Too much difficulty is encountered through unavoidable discrepancies in nomenclature without departing from the more or less standard usages. The letter W has been used for this quantity or, more commonly, $Q\,w$.

While questioning these few minor details the writer wishes to concur wholeheartedly with the basic principles presented in this paper. Very important in utilizing this work is a clear understanding of the underlying assumptions and

[41] "Statistical Theory of Turbulence," by G. I. Taylor, *Proceedings*, Royal Soc. of London, Vol. 151A, 1935.

[42] "Hydrodynamics," by H. L. Dryden, F. G. Murnaghan, and H. Bateman, *Bulletin*, National Research Council, 1932, p. 396.

restrictions applied in the derivation of the theory. Much appreciation is due the author for sifting out these concepts from the mathematical theory of turbulence and presenting those topics of most direct use to hydraulic theory and practice.

SAMUEL SHULITS,[43] ASSOC. M. AM. SOC. C. E. (by letter).[43a]—A need in present-day American hydraulics is to convince the so-called practical hydraulic engineer that fluid mechanics is a tool of much promise and not a scientist's toy. It is gratifying that Professor Kalinske devotes himself so capably to this task and that the Society is "blazing the trail."

Significance of the Statistical Fundamentals.—The frequency distribution curve in Fig. 2 is found to conform to Equation (1), the Gauss-Laplace law of frequency of errors, while other investigators are said to have verified this distribution law. Is it not a well-established mathematical law of statistics that deviations from the mean of any large number of measurements will array themselves in this fashion?

Isotropic turbulence is the name given to a flow situation such that $\overline{u^2} = \overline{v^2} = \overline{w^2}$ and $\overline{u\,v} = \overline{u\,w} = \overline{v\,w} = 0$. The physical significance of this mathematical definition may be of interest. Consider the so-called "precision index,"[44] $h = \dfrac{1}{\sqrt{2\,\overline{v^2}}}$. The larger it is, the more closely do the deviations cluster about the maximum ordinate (of the frequency distribution curve) at which $v = 0$; or the greater is the concentration of the observations about their mean. Since $\overline{v^2} = \dfrac{1}{2\,h^2}$, the first condition of isotropism merely states that, as a whole, each of the three rectangular velocity components departs from its mean value in the same general way.

If u and v are always of the same sign, $U + u$ and $V + v$ have like trends: $u\,v$ and the mean of the products $(\overline{u\,v})$ are always positive. A value of the instantaneous velocity along the x-axis greater than the mean will be found with an instantaneous velocity along the y-axis greater than the mean; the same applies to sub-average instantaneous velocities. A negative $u\,v$ indicates the coexistence of a sub-average and a super-average velocity along the x and y axes—that is, an opposite trend of variation. A positive value of the algebraic sum of these products $(\overline{u\,v})$ stands for a like over-all trend of u and v whereas a negative quantity indicates an opposite course. If the grouping of u and v is quite accidental so that there is no relation between them, the sign of the product $u\,v$ will be negative and positive haphazardly and the mean product of a large number of observations will be zero. Consequently $\overline{u\,v} = 0$ denotes that there is no recognizable relation between u and v, as the plus and minus values of the products are collectively of equal weight. Therefore, the nature of u cannot be predicted from v and vice versa, and their relationship is en-

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[43] Associate Hydr. Engr., Flood Control Div., U. S. Engr. Office, Louisville, Ky.

[43a] Received by the Secretary March 8, 1940.

[44] "Handbook of Engineering Fundamentals," edited by O. W. Eshbach, John Wiley & Sons, Inc. 1936, pp. 2–124.

tirely random, which is what one might envisage in the word "turbulence." Thus the second condition of isotropism is simply one mathematical statement of the disorderedness or "mixed-up-edness" of turbulence.

The transformation of the statistical mean product into a non-dimensional criterion results in the correlation coefficient, R_x. If v_1 always equals v_2, $R_x = 1$, whereas if v_1 is always the negative of v_2, $R_x = -1$; but $R_x = 0$ signifies no apparent interdependence between v_1 and v_2—their array (arrangement) is entirely accidental. When v_1 and v_2 are observed at points very close together (within the average size eddy), a close relationship (or correlation) should be expected, so that R_x should be nearly unity as Professor Kalinske states. The smaller x is, the more alike v_1 and v_2 should be, a situation that is easily visualized. On the other hand, the farther apart the points are, the more random should be the mathematical relationship, and the condition of seeming confusion (turbulence) prevails. Thus $R_x = 0$ is merely another parametric statement of turbulence. The physical connotation of Professor Taylor's characteristic scale of turbulence[6] (Equation (4)) follows easily: l' is the distance between two velocities when R_x approaches zero or the disconnectedness of the velocity concepts is absolute. The trend between R_x and x in Fig. 3 is also explainable in the light of the foregoing, although not the exact equation of the curve.

Professor Taylor's premise that "the statistical correlation between the velocity of a fluid mass at one instant and at another later instant is high when the time interval is small, and this correlation approaches zero when the interval becomes large," is an easily accepted physical dictum. The shorter the time interval between two measurements at one point, the more related they might be expected to be and therefore the closer R_t would be to unity. As the interval increases the relation should become more haphazard, and R_t approaches zero. Thus, the fundamentals of the statistical correlation method are used to express physical realities of turbulence.

The Diffusion Coefficient.—The coefficient of viscosity, μ, has the dimensions $\frac{F\,T}{L^2}$, or a unit force times time. In pure viscous flow the unit shearing stress or unit frictional force is completely given by $\tau = \mu \frac{du}{dy}$; but in turbulent flow, there is an additional friction or drag due to the transverse motion of the water particles. By analogy this frictional force has been expressed as $\tau' = \mu' \frac{du}{dy}$ in which μ' is an apparent viscosity. The total unit friction is then as expressed by Equation (7). Since $\mu' = \rho\,\epsilon$ and ρ has the dimensions $\frac{M}{L^3} = \frac{F\,T^2}{L^4}$, then ϵ has the dimensions $\frac{\frac{F\,T}{L^2}}{\frac{F\,T^2}{L^4}} = \frac{L^2}{T}$, a velocity times a length, or an area di-

[6] "Statistical Theory of Turbulence," by G. I. Taylor, *Proceedings*, Royal Soc. of London, Vol. 151A, 1935, p. 421.

vided by time. Its proportionality to D, the diffusion coefficient of Equation (16), is readily seen. The German name for ϵ is "Austausch-coeffizient" or exchange coefficient, which is preferable to the artificial "mechanical or eddy viscosity," especially since ϵ does have the dimensions of a viscosity. It is really a time rate of change of area and therefore its significance as a measure of transverse diffusion or particle transfer is apparent.

Whither Fluid Mechanics?—From about the middle of the nineteenth century to the present there may be said, without rigid accuracy, to be three phases in the development of hydraulics and fluid mechanics. The first period was a very prolific stage of empiricism (Bazin and Darcy, for example) to which the practical hydraulic engineer is indebted for much of what is still the most serviceable information. Hydrodynamics (Boussinesq and Lamb, for example) existed contemporaneously, but the hydraulician and the hydrodynamist do not seem to have collaborated. The former was prone to regard the latter as impractical and given to making premises not related to the facts. This first phase is still a powerful and indispensable overlapping one, from which modern fluid mechanics will derive much of its basic data.

Then followed a short second phase with Prandtl the leader, in which analogies and imaginative concepts were used, with the results of aerodynamic research as a basis. The main accomplishment was to coordinate and correlate old and new experimental data, with particular emphasis on Reynolds' number and dimensional analysis. Much basic data were gathered.

The present is the beginning of the third phase. As far as fluid mechanics is concerned, the other two periods had not been very fertile of broad theories, although productive of much data. The researchers suddenly realized that the experimental results must be regarded as arrays of fluid mechanics statistics if there was to be order in the research process and in the interpretation and synthesis of the information into basic concepts. The recent statistical theory of turbulence is more accurately the embryonic theories and facts of turbulence examined in the light of the laws of chance or statistics. It is not new theory—only the dawn of orderly research. The shame and wonder is that investigators were so late in their realization of this gross inadequacy in their methods. A debt of gratitude must be expressed to Professor Taylor for impressing the facts forcefully on modern hydraulics and to Professor Kalinske for his masterful applications.

Even the modern hydrologist does not seem to realize that hydrology is a collection of hydrologic facts requiring statistical analysis. There is very little theory to modern hydrology—merely an interpretation of data.

There is a great need for research of the kind Professor Kalinske has accomplished. With the present meagerness and paucity of basic laboratory and field data, statistical methods will avail very little. The statistical theory of turbulence does not obviate the observation and measurement of turbulent phenomena.

Corrections for *Transactions:* In *Proceedings*, October, 1939: page 1388, line 18, change "vary with time" to "vary irregularly with time"; page 1390,

line 5 following Equation (2), change "turbulence in a hydraulic jump" to "turbulence beyond a hydraulic jump"; page 1394, line 3 following Equation (8), change "$\left(\frac{\partial y}{\partial x}\right)^2$" to "$\left(\frac{\partial u}{\partial x}\right)^2$"; page 1396, line 22, change "$\sqrt{\overline{\overline{v^2}}}$" to "$\sqrt{\overline{v^2}}$;" page 1399, line 10, change "V" to "v"; change footnote 14 to read "Investigation of Errors in Pitot Tubes," by C. W. Hubbard, *Transactions*, A. S. M. E., Vol. 61, 1939, p. 477"; page 1402, line 18, change "energy transformation can be increased by increasing," to "energy transformation can be increased, assuming Q constant, by increasing"; and in footnote 16, change the title of the paper to read "Experiments on the Mechanics of Sediment Suspension." Furthermore, change "w" to "γ" in the following places: Page 1392, lines 4 and 5 preceding Equation (5); and in Equation (6) and line 1 following Equation (6).

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DISCUSSIONS

PROBLEMS AND TRENDS IN ACTIVATED SLUDGE PRACTICE

Discussion

BY MESSRS. FRANK C. ROE, AND GERARD A. ROHLICH AND CLAIR N. SAWYER

FRANK C. ROE,[6] ESQ. (by letter).[6a]—The special topic of porous diffusers as used in activated sludge treatment is not included in Mr. Regester's very excellent digest of the subject. It is believed that the following may therefore be a helpful supplement.

The first diffusers were perforated pipe. Obviously, large air bubbles resulted in low efficiency and there were corrosion troubles. After trying porous wood diffusers in experimental work at Milwaukee, ceramically bonded silica in the form of porous plates was used successfully. Until 1920, relatively fine grades of such porous plates were used on the theory that the small air bubbles diffused permitted maximum absorption with high efficiency.

In the 1920's, clogging troubles with fine-grade porous plates, resulting in high pressure losses (which became prohibitive in some cases), led to the adoption of porous diffusers having a more open structure. By 1930, the efficacy of coarser porous diffusers was apparent, and specifications have since called for permeability ratings of from 30 to 50 as compared with 10 to 15 prior to 1920.

The dry permeability rating of diffusers (cubic feet per minute of free air per square foot under a 2-in. water pressure) which was arbitrarily adopted in the early days for convenience of testing, has undergone refinement with the introduction of coarser, low pressure-loss plates. Obviously, the control of uniform diffusion becomes more delicate as pressure loss is minimized. Individual diffuser units controlled by a single valve must therefore have the same pressure loss within narrow limits to assure uniformity.

Diffuser manufacturers found that the temperature of plates and testing air must be controlled to avoid errors. Diffuser specifications should be so written that such control is exercised.

NOTE.—This paper by Robert T. Regester, M. Am. Soc. C. E., was published in November, 1939, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: March, 1940, by Langdon Pearse, M. Am. Soc. C. E.

[6] Sales Mgr., Porous Products and Laboratory Ware, The Carborundum Co., Niagara Falls, N. Y.

[6a] Received by the Secretary March 4, 1940.

Diffuser tubes have proved equally effective in comparison with diffuser plates. In some instances, the assembly and installation of diffuser equipment favors tubes over plates on a cost basis. In general, tubes have proved most beneficial in small plants because of the ease of servicing diffusers. However, tubes have been adopted for a large plant at Gary, Ind., on the basis of highest over-all economy.

Under the heading, "Needed Study and Research," Mr. Regester recommends studies on maintaining low normal pressure losses through diffusers. Efforts have been made in several instances along this line. Regular addition of chlorine to air proved effective at Charlotte, N. C. One of the most recent promising discoveries is the process of sandblasting the surface of diffusers where clogging is severe.

GERARD A. ROHLICH,[7] JUN. AM. SOC. C. E., AND CLAIR N. SAWYER,[8] ESQ. (by letter).[8a]—The compilation of data relating to the design and operation of activated sludge plants in the United States is one of the valuable contributions of this paper and is worthy of the interest and attention of engineers, operators, and research men concerned with the activated sludge process.

As stated by Mr. Regester, information obtained from the design of large plants such as those at Milwaukee, Chicago, and Indianapolis, has guided the design of subsequent works. However, difficulties have been encountered in the operation of many of the smaller plants, and even in some of the larger plants, which have attempted to lean heavily on information gained at some other large city. This indicates strongly that the problems of each city are different and require "custom-made" rather than "ready-made" plants. The writers agree with the statement that "The need for additional study and research is evident." A fuller understanding of the basic principles of the process is of vital importance if the design of activated sludge plants is to be placed on a rational basis.

A factor not mentioned by the author that the writers believe to be an important design consideration is the effect of temperature on the rate at which activated sludges use oxygen. An extensive laboratory investigation of this factor has been made and the data presented elsewhere by the writers.[9] The change in temperature which occurs in most activated sludge plants in temperate climates between the extremes of late summer and late winter is usually in excess of 10° C. Simultaneous studies made on identical mixtures of activated sludge and sewage maintained at 10°, 15°, 20°, and 25° C reveal that the highest rates of oxygen usage are obtained under summer conditions (25° C) and the lowest rates under winter conditions (10° C). The most rapid stabilization is accomplished under summer conditions, and thus a much shorter period of treatment suffices at the higher temperatures for the sewage-sludge mixture to reach the base rate of oxygen utilization, which is indicative of stabilization of the organic matter in the sewage. At the lower temperatures the maximum

[7] Instr., Civ. Eng., Carnegie Inst. of Technology, on leave; Hydr. Laboratory, Univ. of Wisconsin, Madison, Wis.

[8] Post Doctorate Fellow, Hydr. and San. Eng., Univ. of Wisconsin, Madison, Wis.

[8a] Received by the Secretary March 7, 1940.

[9] "The Influence of Temperature Upon the Rate of Oxygen Utilization by Activated Sludges," by C. N. Sawyer and G. A. Rohlich, *Sewage Works Journal*, Vol. XI, No. 6, November, 1939, p. 946.

rate of oxygen usage is much lower in magnitude, and therefore is prolonged for a greater period of time. Thus a longer detention period in the aerators is required under winter conditions to accomplish the same degree of sewage oxidation or stabilization. For plants operating with fairly uniform concentrations of aeration solids throughout the year, the distribution of air should be varied with the changing temperature to fit the varying oxygen-utilization rate curve, if most efficient plant operation is to be obtained. In order to take full advantage of this variable a plant must be equipped with extremely flexible aeration facilities.

The investigation previously reported[9] covered a series of studies made on activated sludges obtained from four cities and having flows varying from 450,000 gal per day to 120,000,000 gal per day. The 5-day B. O. D. strengths of the sewages being treated by these activated sludges varied from 140 ppm to 530 ppm. The results showed that the rate of oxygen usage at 20°, 15°, and 10° C was approximately 72%, 45%, and 25%, respectively, of the rate shown at 25° C, and the small divergence in the results obtained on the different sludges led to the proposal of the equation $y = 0.71\ x^{1.54}$, in which y is the relative activity expressed as a percentage of that at 25° C and x is the temperature in degrees centigrade.[10] If the over-all amount of oxygen used is a measure of the amount of biochemical oxygen demand stabilized, the aeration period during the cold winter months should be approximately three to four times as long as in the hottest part of the summer in order to attain an equal degree of stabilization of the activated sludge.

This is only one of the effects of temperature on the activated sludge process that the engineer must acquaint himself with if his design is to be based on fundamental knowledge, rather than on what is the accepted practice. Other factors worthy of consideration are the effect of temperature on (1) the rate of growth, (2) the volatile solids content, and (3) the nitrogen content of the sludges. These factors will be of importance in determining digester capacity, gas production for power purposes, and also in the design capacity of units used in those cases where filtration and drying of the waste activated sludge, for incineration or fertilizer use, is practiced.

Mr. Regester's statement that (see heading "Operation and Control of Process: Suspended Solids in Mixed Liquor") "The optimum suspended-solid content of the mixed liquor in the aeration tanks is best determined by actual plant performance" substantiates the seventh item in his stimulating list of phases of the activated sludge process requiring needed study and research. This item states the need for investigation showing the possible general relation between the optimum suspended-solids content of aerated mixtures and the character of the settled sewage. To gain a concept of the significance of variation of suspended solids on the oxygen requirements of activated sludges, studies[11] in 1938, have shown that, as the concentration of activated sludge increases, the rate of oxygen utilization also increases in nearly direct proportion to the concentration of sludge used. With high rates of oxygen usage pro-

[10] "The Influence of Temperature Upon the Rate of Oxygen Utilization by Activated Sludges," by C. N. Sawyer and G. A. Rohlich, *Sewage Works Journal*, Vol. XI, No. 6, November, 1939, Fig. 3, p. 960.

[11] "Effect of Sludge Concentration and Temperature Upon Oxygen Utilization," by C. N. Sawyer and M. Starr Nichols, *loc. cit.*, Vol. XI, No. 1, January, 1939, p. 51.

duced by high sludge concentrations, the break in the rate curve,[12] indicating the completion of a certain phase of the oxidation, occurs most rapidly and the rate drops to a low level very quickly. This means that with high sludge concentrations the degree of stabilization needed for satisfactory treatment of the sewage is attained much sooner and that a shorter aeration period is sufficient, whereas a longer aeration period is required in the case of the slower rates of oxidation given by low suspended-solids concentrations to obtain an equal degree of stabilization.

It is interesting that the effect of increasing the concentration of aeration solids gives oxygen-usage results which are comparable with those obtained by increasing the temperature when the solids concentration remains constant. The long period of treatment required for stabilizing the sewage at low temperatures can be shortened materially by increasing the aeration solids during such periods. Thus, by varying the sludge concentration with the season or temperature, the degree of stabilization necessary can be accomplished under all conditions of temperature during the normal aeration period provided. To the knowledge of the writers, no activated sludge plants are operated on this basis at present. With the sludge concentration being varied as the temperature changes, the oxygen-utilization characteristics of the aeration mixture will remain much more uniform and less flexibility will be required in the aeration facilities. The application of these principles may prove valuable to some existing plants which have very little flexibility in their aeration devices.

The need for basic information on the optimum nutritional requirements of activated sludge and on the behavior of sludges fed on unbalanced diets cannot be stressed too greatly. Any engineer who has had a hand in the design and operation of a sewage treatment plant for a small city of less than 10,000 population realizes that the problems to be solved in the small city are just as numerous as, and perhaps more critical than, those in a large city. The influence of specific wastes in sewage on the ultimate behavior and operation of the treatment plant is of concern to all. Such fundamental information as can be obtained from basic nutritional studies on activated sludge should be of considerable value in determining the applicability of the activated sludge process to the disposal of industrial wastes that may lend themselves to biological treatment.

The current trend of the federal government toward entering the field of stream-pollution control is certain to tax the present limited knowledge of sewage and waste disposal to the utmost; and, with the author's thought-provoking list of items needing further study and research, the engineer has before him a challenge that he will have to meet not only on engineering ground but on the fields of allied subjects such as chemistry, biochemistry, and bacteriology as well.

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[12] "Effect of Sludge Concentration and Temperature Upon Oxygen Utilization," by C. N. Sawyer and M. Starr Nichols, *Sewage Works Journal*, Vol. XI, No. 1, January, 1939, Graph 4, p. 57.

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DISCUSSIONS

BRIDGE AND TUNNEL APPROACHES

Discussion

BY MESSRS. GEORGE H. HERROLD, PARK H. MARTIN, AND CHARLES M. NOBLE

GEORGE H. HERROLD,[12] M. AM. SOC. C. E. (by letter).[12a]—Smooth uninterrupted traffic flow is the ultimate goal on all traffic routes. This is being attained by slow degrees. A valuable contribution is the special research, by Mr. Curtin, of the studies that have been made of the vehicular approaches of some of the outstanding tunnels and bridges of large carrying capacity built in recent years. In fact, it is a treatise on the subject because it brings together knowledge by description that heretofore had to be looked for under many sources. The idea of treating bridge and tunnel highway approaches as a subject separate and distinct from general highway construction is to be commended because these approaches have their own distinct problems. The solution of an approach problem must stand forever, either impeding or increasing the efficiency of the bridge or tunnel.

Traffic fatalities cannot be stopped by legislation or law enforcement or committee meetings, although it is through these mediums that a very valuable educational program may be conducted, the ultimate result of which will be new engineering devices for building safety into highways. To insure smooth uninterrupted traffic flow, it is necessary to build into approaches some necessary features that will make it impossible for the careless driver to make abrupt changes in direction or other unexpected or irregular movements, and that is the principle that has been followed more or less effectively in the approaches studied.

Approaches to confined roadways such as bridges and tunnels should not, in any way, limit the capacity of the structure which, because it has no cross-traffic in its own length, has a capacity of some 1,500 vehicles per lane per hr. Signal installations on thoroughfares at the ends of such structures, however, are likely to cause great trouble as the carrying capacity of signalized thoroughfares is only 600 to 800 vehicles per lane per hr. It is to be noted that many of

NOTE.—This paper by John F. Curtin, Jun. Am. Soc. C. E., was published in November, 1939, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: February, 1940, by Messrs. Dean G. Edwards, and John W. Beretta; and March, 1940, by George Hartley, Esq.

[12] Planning Engr., City Planning Board, St. Paul, Minn.

[12a] Received by the Secretary March 4, 1940.

the structures that have been studied are of the self-liquidating type, indicating, perhaps, that more attention has been given to approaches where tolls are charged than to approaches to tunnels and bridges which have been paid for through taxes. Although the author deals with large structures, the same principle applies to smaller ones and many of these can have their life's usefulness extended and improved by using the data collected by the author.

Mr. Curtin mentions the studies made by Mr. Evans,[3] in which he made use of certain principles laid down by railroads. It is interesting to note this gradual turn toward railroading principles and the use of tapering converging lanes. A railroad train travels in a lane on rails and can change that lane only at stated intervals; and this may be the ultimate principle that will control the handling of vehicular traffic.

The author makes no mention of city planning or the necessity of a complete coordination of movements of vehicles to, and from, bridge and tunnel approaches and the thoroughfare plan of the city. The influence of the Regional Plan of New York, however, can be found in the New York City entrance to the Holland Tunnel and in the New Jersey and New York approaches of the Lincoln Tunnel. Its influence can be found in other approaches, although the approach itself may not have been perfect because, as the author states, vested interests and expensive demolitions may prevent following the plan.

PARK H. MARTIN,[13] ESQ. (by letter).[13a]—The subject of bridge and tunnel approaches has been covered ably in this paper. The fact that 25% to 50%, and sometimes more, of the cost of modern bridge and tunnel projects is in the approaches, is well established. With this premise accepted, it should follow that in selecting the site for the project, the engineer should consider the topography, in relation to approach planning, with the same care as he considers traffic volumes, traffic collection, and distribution.

Some sites may offer advantages of one kind over advantages of another kind at another site. The topography of one site may suit one type of approach and not be fitted for another type. It seems pertinent to emphasize the fact that, in selecting the site, the engineer should have in mind all the vital elements affecting the project—type of structure, foundations, clearances, type of approach, traffic, property damage, and effect of the structure on the district. The location selected for a non-toll project may be entirely different from one selected for a toll project.

The major part of Mr. Curtin's paper is devoted to the type of project requiring toll facilities. The non-toll, direct street and highway approach receives much less consideration than the reservoir and plaza toll types, although the direct street and highway approach types are far more numerous. The economies involved in the problem of the type of approach are all on the side of the direct highway connection. The economics of the entire subject receives only passing mention in the paper.

[3] "Approaches to River Crossings," by J. C. Evans, an address at Harvard University, May 13, 1938 (not published).

[13] Planning Engr., Allegheny County Planning Commission, Pittsburgh, Pa.

[13a] Received by the Secretary March 6, 1940.

Undoubtedly it is true that the direct or highway extension approach lends itself more readily to rural and suburban areas for two reasons—lesser traffic volumes, and costs. If immediate needs are satisfied by the direct approach and if estimated future requirements indicate the need later of a traffic separation device at certain points, it would seem advisable to acquire sufficient right of way in the first instance for use in the ultimate.

The point of weakness in the direct approach is usually at the intersecting main highways that may be feeders to the bridge or tunnel. This fault may be corrected by use of traffic-separation devices or traffic circles. A simple traffic-separation device at an important arterial route, as the beginning of a street extension approach, may be all that is required to make the approach functionally adequate.

The theory advanced by Mr. Curtin that the use of traffic circles is limited to suburban and rural areas, in connection with approach planning, because of the area required and cost, is not borne out in the writer's experience. A type of grade separation that might perform, as effectively, the work of a traffic circle will require almost as much, or more, area as the circle, with a greater physical cost. This is particularly true when the intersecting highways are in a flat area. The traffic circle will also have advantages if there are more than two highways to be accommodated at the same intersection.

The final decision as to the type of device to be used must be based on the functional adequacy of the device, with due consideration to the cost. On a non-toll project, the approach has only one function—to provide adequate access to the bridge or tunnel. On a toll project there is the added function of toll-collection facilities, which must be ample to permit a reasonably free movement of traffic. In either case, the estimated traffic volumes should, in a large measure, decide the type of approach. On a toll structure, storage capacity from the collection booths to the first cross or intersecting highway must be ample, if a separation device is not used. If the distance is too short for storage to accommodate peak traffic volumes with the ordinary number of traffic lanes, then additional lanes in the form of a tapered plaza should be considered. By extending the additional lane or lanes to the intersection, in a case of no separation device, the capacity of the intersection by light control can be increased greatly.

The tapered plaza at the New York City end of the Lincoln Tunnel functions as a direct approach. The wide entrance collects the traffic, then by means of converging traffic lanes, channelizes the traffic into the two lanes of the tunnel. This principle of additional traffic lanes at intersections has been used in highway construction. An interesting example of this type has been built in Lake County, Indiana, where U. S. Route 30, the Lincoln Highway—a 40-ft highway —crosses U. S. Route 41—another 40-ft highway.[14]

The diamond-shaped toll plaza of the San Francisco-Oakland Bay Bridge as shown in Fig. 2, is located on a form of a direct highway approach. The only justification for the diamond-shaped plaza is to accommodate toll facilities.

The tapered plaza is a close relative of the square or rectangular plaza. In the writer's opinion, both have their advantages, considering street patterns

[14] *Concrete Highways*, September–October, 1938.

and traffic volumes from various feeders. Inherently they both work on the same principle of collection and storage from feeder connections.

One of the serious problems affecting either type is unbalanced feeder loads, this problem being particularly serious where toll collection is required.

The reservoir approach is a street-extension approach with a wide throat in the form of a rectangle or square, acting as a traffic collection well, with large storage capacity. Its use belongs in urban areas with heavy traffic volumes and may well be applied to both bridges and tunnels. The shape and size will be governed by street patterns and traffic volumes.

Mr. Curtin illustrates certain weaknesses in the operation of the reservoir plaza at the New York end of the Holland Tubes. With the limited information given, as to traffic volumes, it would seem as if the difficulty is nothing inherent in the reservoir type of approach, but rather a volume of traffic at the peak hour that exceeds the capacity of either the collection booths or the tunnel. Again it may be that traffic density on one of the approach streets may be greater than the collection booths on that side of the plaza can accommodate.

The author's further criticism of the reservoir plaza, as being ill-adapted for use as storage space, does not seem to be warranted. This may be no inherent fault of the rectangular or square plaza, but rather in the design of the exits or throat from the plaza into the tunnel.

The use of transition lanes of sufficient length from the plaza to the tunnel should correct this condition. In support of this solution, the writer would refer to the following statement in the paper (see heading, "Definitions: Reservoir Approaches"): "In addition, the twelve radial lines of cars must narrow into two lanes in a relatively short space."

The author criticizes the use of the rectangular or square plaza as being uneconomical in a business or industrial area, due to inability to use all of the space in the plaza because of the shape. The writer does not agree with the author on this point. If the device, whether it is a rectangular, square, or tapered plaza, whether it is a direct street or highway approach, with or without traffic separation devices, functions adequately for the purpose designed, then it should not be questioned. Any other device or type of approach which will not perform the function for which designed, with or without unused space, has no right to be considered.

For a non-toll bridge there is a type of approach that should be of interest to engineers. A particular case, with which the writer has been associated, involves the Highland Park Bridge across the Allegheny River, with a main highway at each end, both at right angles to the bridge, in what is rather a confined area due to physical conditions. The bridge, a non-toll structure, shown in Fig. 14, has been constructed by Allegheny County, Pennsylvania. Both highways are four-lane arterial routes leading to and from the City of Pittsburgh, Pa., each with traffic volumes in summer of more than 5,000 cars per day. The bridge is built at an elevation to pass over the highways, with approach treatment the same at each end. After passing over the highway, the approaches curve in general through 90°. Then they run parallel to the main highway until the approach grade is common with the grade of the main highway. The approach is finally curved to an intersection with the main highway. The ap-

proaches provide for on and off movements from the bridge, with one movement on the highway, with no crossing of traffic. At a point just before the bridge crosses over the main highways, ramps are constructed to the right and left from the bridge to the highway, thus providing access to and from the bridge from the opposite traffic lanes on the main route. The bridge was opened to traffic in June, 1939, and there have been no delays or traffic tie-ups at any time.

The principle of this separation is being used on highway construction and is well adapted to bridge approaches of this type. A striking example of its use on highway construction is on the Pennsylvania Turnpike mentioned by the author following Equation (4).

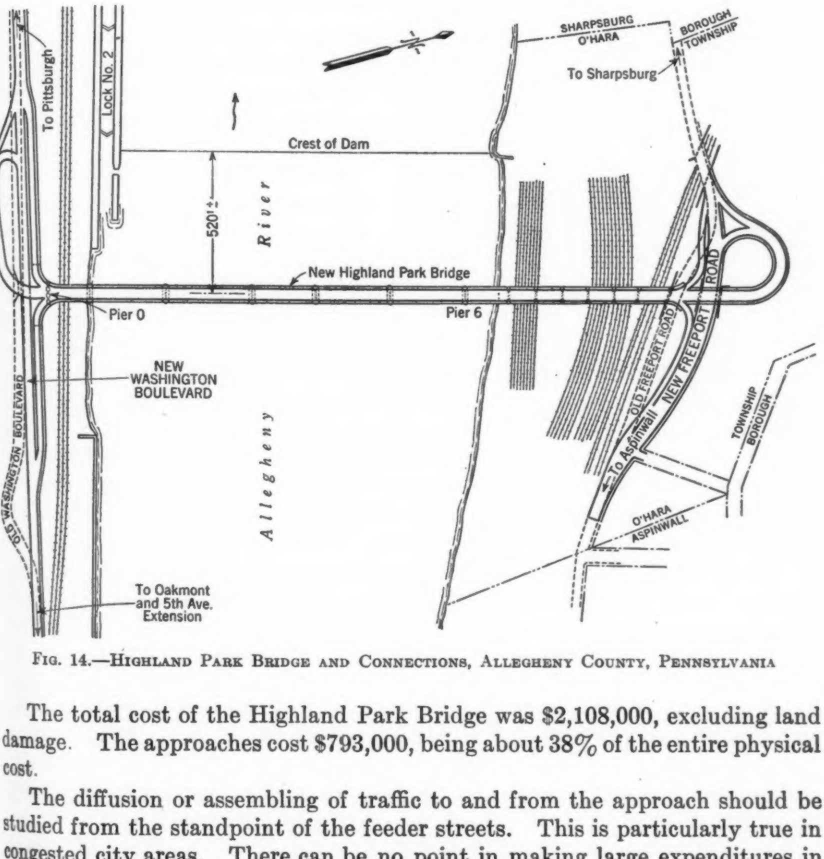

FIG. 14.—HIGHLAND PARK BRIDGE AND CONNECTIONS, ALLEGHENY COUNTY, PENNSYLVANIA

The total cost of the Highland Park Bridge was \$2,108,000, excluding land damage. The approaches cost \$793,000, being about 38% of the entire physical cost.

The diffusion or assembling of traffic to and from the approach should be studied from the standpoint of the feeder streets. This is particularly true in congested city areas. There can be no point in making large expenditures in relieving one spot to the detriment of others.

Under the heading, "Definitions: Direct Street or Highway Extension," Mr. Curtin mentions the Liberty Tunnel at Pittsburgh. The situation existing at the north or downtown end of the Liberty Bridge, which connects directly to the Liberty Tunnel, is a striking example of the need of traffic diffusion (see

Fig. 15). The present bridge approach is a direct street approach, with the terminus at Forbes Street. There are three connections between Forbes Street and the bridge proper. Traffic using these three connections does not represent more than 25% of the bridge traffic. The remaining 75% concentrates at the Forbes Street and bridge approach intersection and 40% of the total bridge traffic has no need or desire to find its way through various streets to and from the Forbes Street intersection. What is needed and planned is a separation of traffic at Forbes Street by means of a crosstown highway, extending to the north edge of the Triangle, separated from all cross streets by means of overpasses and underpasses. The plan, so arranged to allow sufficient points of distribution and access, warrants the belief that it is functionally adequate without superimposing excessive traffic loads at any one point. A complete origin and destination survey of traffic using the Liberty Bridge was necessary to arrive at an intelligent analysis of the problem. The estimated cost of the Crosstown Highway is $7,000,000.

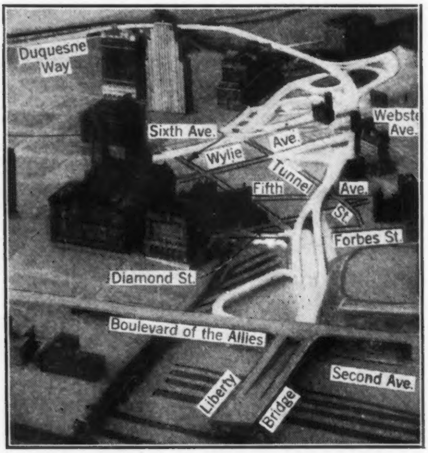

FIG. 15.—MODEL CROSSTOWN HIGHWAY, PITTSBURGH, PA.

This cost, with the cost of grade separations planned at both ends of the Liberty Tube, will show a total expenditure of $10,000,000 to correct the conditions. The total cost of the Liberty Bridge and Tunnel was $13,000,000. Thus, with the additional contemplated expenditure of $10,000,000, there is, again, more the 40% of the cost of the entire project in the approaches and connections.

Mr. Curtin recommends that in regard to approaches, serious consideration be given to the steady-flow system as developed by Fritz Malcher.[5] The use of elongated separating islands 10 ft to 40 ft wide is recommended. Without any thought of criticizing the general theory of the steady-flow system, the writer will severely criticize the use of any island as part of a steady-flow system that does not provide for a minimum turning radius of 30 ft. The minimum width for the island should be 50 ft. Part of the principle involved in the steady-flow system is a form of a U-turn. Any form of a U-turn is subject to some criticism, and with an island 10 ft wide the U-turn is fatal to the free-flow system.

Use of the freeway or express highway is commended as being worthy of the utmost consideration. Physical conditions and cost should be the only factors against its use. It represents the most advanced thought on traffic movement.

Design of Approach Connections.—Under the heading, "Approach Connections: Design of Approach Connections," Mr. Curtin has discussed various

[5] "The Steady Flow System," by Fritz Malcher, Harvard Univ. Press, 1935.

factors that should be considered, such as design speed, sight distance, curvature, grades, and lane width. All of these elements are factors in arriving at the completed plan.

The writer feels that Mr. Curtin has confused his discussion, to some extent, by treating both the approach and the approach connection the same. The design of the approach, particularly if it is one of the type of an arterial highway, may be entirely different from that of an approach connection. Physical limitations, such as existing street patterns, buildings, property damage, and other elements, may force the use of a lesser speed, shorter sight distance and steeper grades on approach connections than would be desirable on the main approach. The writer's experience in such matters in congested areas is such as to convince him of the necessity of considering the approach connection as somewhat different from the approach. Again the fact that the entrance of the vehicle from the approach connection on to the approach is generally made after a stop has been required, indicates that the speed limitation should be lower, rather than equal to the speed limitations on the approach. It follows, therefore, that if speed is a minor element in the approach connection, grades may be increased, curvature sharper, and sight distance less. Sharper curves may require wider traffic lanes. As an illustration, in Allegheny County it has been necessary, on approach connections, to have curve radii as low as 80 ft. In such cases the minimum lane width used is 12 ft. Attempt is made to fix the maximum grade at 5%. This gradient is only exceeded when forced by physical conditions.

The writer must take exception to Mr. Curtin when, under the topic of "Approach Connections: Wye Junctions at Connections," he states: "It is desirable to reduce, as much as possible, the slowing down of traffic which results when streams of vehicles divide or join at the intersection of two roadways." It must be remembered that the maximum capacity of any highway is reached at a vehicular speed of 23 to 25 miles per hr. Safety alone should demand a slowing down of vehicles at such wye junctions. Actual practise on most highways properly supervised requires that one of the converging highways be a "Stop" point. The engineer may discuss streams of traffic and fluidity of movement, but the fact will still remain that he cannot mix two streams of automobiles as one mixes streams of water.

The latter part of the paper, covering "Plazas: Provisions for Toll Collection Facilities," "Plazas: Provisions for Convergence of Lanes" and other topics, seems to the writer to cover, fully, the field of the particular topic.

From an engineering standpoint, the paper is excellent. However, to the writer it is only the first of two chapters on the subject. The subject is not completely covered without a discussion of the economics. Substantially all the projects discussed by Mr. Curtin are toll structures, many financed in part by federal grants. For those who must deal in planning projects of similar nature, with or without tolls, without federal "largess," the economics of the problem are as vital as any other element.

Charles M. Noble,[15] M. Am. Soc. C. E. (by letter).[15a]—In the past, many unrelated groups of engineers have been called upon to design bridge and tunnel

[15] Special Highway Engr., Pennsylvania Turnpike Comm., Harrisburg, Pa.

[15a] Received by the Secretary March 7, 1940.

approaches and each group has solved the problems based entirely upon individual judgment without the benefit of being able to refer to comparative criteria. Consequently, the results have reflected this individuality and in many cases undesirable nonuniformity has resulted which could have been eliminated had literature stating fundamental principles been available.

Mr. Curtin is to be congratulated on his able and comprehensive presentation of the factors entering into the proper design of bridge and tunnel approaches. There is little or no literature on the subject and for the first time comparative criteria have been set up in easy reference form for further study by members of the profession.

The paper is notable in that it delineates the fundamental requirements entering into the design of approaches for motor traffic. It treats details sufficiently so that a base line is established from which future designs may be developed, and it furnishes a medium through which this particular art may be developed upon a more scientific engineering basis. In the advancement of any art toward a scientific approach it is first necessary for someone to make available, to the practitioners of that art, literature in which first principles are stated and fundamental criteria formulated. After this is done, and as time passes, these fundamentals may be restated in more precise form and the criteria revised to more nearly approach a perfect state. Only in this manner and through this practical development can principles of design be established on a more scientific foundation, and the writer feels that Mr. Curtin has performed a distinct service to the profession by the presentation of this paper.

There can be little doubt that the provision of adequate and ample approaches to a bridge or tunnel is fundamentally sound. The truth of this has been fully demonstrated in almost every large city where vehicular crossings have been constructed without regard to suitable approach facilities and where the economic loss due to incomplete use of the structure itself and to the congestion occasioned in the adjacent street system can be readily seen. To the writer's knowledge, O. H. Ammann, M. Am. Soc. C. E., was a pioneer in recognizing that the design of the approaches to vehicular crossings was of equal importance to the design of the main structure itself and that it was essential for sufficient money to be expended on approaches in order to accomplish, fully, the ultimate objectives of the project. Mr. Ammann enunciated these principles in 1925 during the early stages of the design of the George Washington Bridge, and consistently followed this policy in the design of the subsequent structures with which he was connected as chief engineer. The result may be observed on the George Washington Bridge, the Bayonne Bridge, the Outerbridge Crossing, the Goethals Bridge, the Lincoln Tunnel, the Triborough Bridge, and the Whitestone Bridge—all in the metropolitan area of New York City.

As Mr. Curtin states, the development of a satisfactory approach and plaza system for a bridge and tunnel crossing depends on an exhaustive analysis of all the factors surrounding each particular site, such as traffic volume (direction and intensity), topography, property values, and the presence of utilities, such as railways, subways, aqueducts, and other public services. Indeed, the "cart" must often "come before the horse," for the suitability of contiguous areas for

the proper development of an approach system should be determined, in a general way, before the site of the main crossing is selected. In order that the over-all economic health of the community may be preserved most completely, it is most desirable that consideration also be given to the effect of the new facility on adjacent property. Often, complications in the development of approach plans have been the cause of the rejection and elimination of alternate locations from further consideration.

Experience in the past seems to indicate, in general, that elevated structures have a tendency to depreciate surrounding property values; whereas depressed construction apparently has no adverse effect, and in some instances an improvement in property values has occurred after completion of the project. This is understandable when the limitations on light and air imposed by an elevated structure are considered. A correspondingly favorable effect is produced by depressed construction, which involves clearing away buildings and results in an open wide space, thereby improving light and air conditions. This does not mean, however, that elevated construction has no place in the design of highway facilities, but it does mean that the engineer in charge should be fully awake to the broad implications involved, in order that the problem will be studied, not only from an engineering and traffic standpoint, but also from the viewpoint of the economic and social effects on the community at large.

The origin and destination traffic studies, the analysis of the capacity of existing traffic arteries and prognostication of future requirements outlined by the author together with comparative cost estimates, a survey of the probable effect of the improvement on the tax structure, and the social and economic effects on the community in particular and at large constitute the only intelligent method of determining the specific location and the general design of the approach system.

Mr. Curtin analyzes correctly the disadvantages of the type of reservoir plaza in which the toll booths are arranged in semicircular form and, in comparison, stresses the advantages of the tapered plaza. He advances the idea, however, that the tapered plaza evolved from the design of the advance yard in railroad practice. It is possible to apply the reasoning behind the advance yard to tapered vehicular plazas but as a matter of fact the application of the reasoning came after the development. In the case of the George Washington Bridge the general shape of the New Jersey plaza had been determined as early as 1925, which was considerably prior to the application of railroad usage to this particular problem. The tapered plaza is a natural outgrowth of the obvious flaring required by the increased width made necessary by the toll booths. If there is a street or highway crossing adjacent to any form of plaza, sufficient storage space should be provided so that traffic during peak hours does not "back up" across the intersecting street and block it.

It is believed that the relation of width-to-length in designing converging lanes was first consciously recognized when engineers were designing the New Jersey toll plaza for the Lincoln Tunnel. Unfortunately, no comparative criteria were available and it was necessary to proceed by using engineering instinct and judgment, since, to the writer's knowledge, no scientific method

had been devised up to that time. Mr. Curtin has performed a service in presenting his analysis. It would appear, however, that the use of $f = 0.15$ for deducing the forward distance required for lateral movement is somewhat high for this type of movement; and, furthermore, the execution of a true reverse curve by a motorist is a practical impossibility. In addition, in a crowded plaza the opportunity must present itself before the motorist can progress in a lateral direction, and often considerable forward distance has been consumed before he has found a "hole" in the traffic of sufficient magnitude to permit the lateral movement. The process is one of constant adjustment, braking, falling in behind a parallel vehicle and moving laterally a lane at a time. All of this consumes more longitudinal space than would be indicated, either by the friction analysis or by a straight passing operation on the open road. It is during periods of heavy concentrated traffic that the value of the length-to-width principle is demonstrated and, if the length is too short for the width, congestion will develop which will effectively block and tie up traffic, thus reducing the economic efficiency of the main structure. When it is necessary to merge traffic into a single lane, more forward distance is required than when multiple lanes are involved, not only because of congestion during peak flows but also to assure reasonable safety. It is also felt that a convergence to a tunnel will require more distance than for a bridge and that the convergence should be completed sufficiently in advance of the portal to assure safety.

On the New Jersey approach to the Lincoln Tunnel, the maximum lateral movement for vehicles converging to enter the tunnel is 35 ft and this is accomplished in a distance of 490 ft, the full convergence being accomplished in the open plaza 100 ft in front of the portal. The average speed from a standing start at the toll booths to the tunnel portal may be assumed at approximately 30 miles per hr. This length may be compared with the length of 243 ft interpolated from Table 2. Unfortunately, only one tube of the Lincoln Tunnel is in operation and traffic has not yet been sufficiently dense to furnish a definite guide in setting up the ratio of width to length. The maximum lateral movement on the toll plaza of the George Washington Bridge is 48 ft and this is accomplished in a forward distance of 510 ft which compares with a distance of 300 ft interpolated from Table 2. The convergence has the appearance of being quite severe and as yet has not been subjected to a full traffic load, since a width of only 57.5 ft (two 28.75 ft roadways each) has been paved out of the 90-ft width available on the main structure. In entering the tunnels of the Pennsylvania Turnpike it will be necessary to merge two 12-ft lanes into a single 11.5-ft lane and, due to the narrowing of the median strip, the lateral movement amounts to 17.25 ft. The minimum forward distance in which this is accomplished is 700 ft, and an additional distance of 100 ft is provided before reaching the portal, making a total distance of 800 ft from the beginning of the convergence to the tunnel portal. It is planned to post the tunnel approaches for speeds of 35 miles per hr. From Table 2, for a lateral movement of 17.25 ft, a forward distance of 274 ft is interpolated, assuming a speed of 50 miles per hr.

It is suggested that additional study be given the subject of convergence in view of its economic importance, and, if required, a new table prepared to supersede Tables 1 and 2.

In connection with the question of grades it may be pertinent to add to the data in the paper that the maximum grade at toll booths should be limited to 2%, with less if possible, because of the tendency of the vehicle to roll while the patron is paying the fare. No grade exceeding 2% was used at the toll booths of any of the structures constructed by The Port of New York Authority.

In making provision for toll collection facilities the usual criterion is based on the peak-hour volume expected during the life of the structure as a toll venture. Many facilities will never attract, during this time, a volume of traffic that will result in an intensity of 1,500 vehicles per lane per hr. The traffic simply may not be available. Again, if traffic forecasts indicate that it will be many years before such a concentration occurs, but that such an intensity will occur during the toll life of the project, provision can be allowed for future expansion of the toll area in the initial design, but at first, only sufficient lanes to accommodate the peak-hour traffic during a period of fifteen or twenty years may be provided. Usually, however, the savings realized from such an expedient are rather meager.

In computing the available traffic capacity of the main structure it may be erroneous to multiply the number of lanes by a value of 1,500 vehicles per lane per hr because practically all authorities agree that, as the number of lanes increases, the lane capacity decreases. In addition, most peak hours are unidirectional and one half of the structure may be congested, whereas on the other half traffic may be comparatively light. It is also true that when high intensities develop, there is always agitation by the public for the construction of additional crossings to relieve the congestion.

The toll area for the George Washington Bridge was computed on the basis of a total volume of traffic during the peak hour of 8,400 vehicles for the nine lanes and on the assumption that each toll lane could handle 400 vehicles per hr. On this basis twenty toll lanes were provided, assuming that the surplus 400 vehicles could be absorbed. As Mr. Curtin has stated, actual operating experience indicates that tolls can be collected at the rate of 450 to 600 vehicles per lane per hr, depending on the toll classification structure. It is possible that in the future traffic may increase to such an extent that the toll area of the George Washington Bridge (twenty toll lanes compared with twenty-seven recommended by Mr. Curtin) will be inadequate; but it is worthy of note that an additional crossing, the Lincoln Tunnel, was constructed before any congestion developed on the structure.

That the width of a toll area may sometimes have an important bearing on the total cost of a project is illustrated in the case of the Lincoln Tunnel where the toll area cost $8,500 per ft of width exclusive of buildings and toll equipment.

In addition to the factors mentioned in the paper relative to the necessity of selecting a design speed, the very fact that a design speed has been adopted immediately permits the engineer to think in terms of seconds of time and to plan the layout on the basis of time rather than in feet. Thus, design elements can be thought of as so many seconds apart, instead of so many feet, and a correlation obtained between the motorist's reaction and the design.

Generalized statements, that superelevation on horizontal curves can be limited to three fourths the design speed and that a friction factor of $f = 0.15$

due to unbalanced centrifugal force is acceptable, should be used with caution. For example, if the superelevation is based on zero friction at three fourths the design speed and a speed of 60 miles per hr is selected, the "balanced" speed amounts to 45 miles per hr and a superelevation of 0.142 ft per ft for a 6° curve will result, which is too great a rate for practical use on public highways. If a maximum superelevation of 0.1 ft per ft is assumed for a 6° curve a friction factor of $f = 0.15$ is developed at 60 miles per hr. On the other hand, when a 2° curve is considered (design speed, 60 miles per hr; and balanced speed, 45 miles per hr) a superelevation of $\frac{5}{8}$ in. per ft is required and a friction factor of 0.03 is developed at 60 miles per hr. In this latter case, the superelevation is so great in comparison with the degree of curve that slower vehicles may be induced unconsciously to "edge in" toward the center of the curve; and the friction developed at 60 miles per hr amounts to only one fifth that developed at the same speed on a 6° curve. Since unbalanced friction is the phenomenon which largely influences the driver of a vehicle it would appear very desirable to design superelevation on the basis of a fairly uniform side friction value in order to offer the motorist uniform and consistent operating conditions on curves.

Tests have shown conclusively that, of their own volition, motorists will develop higher friction factors at low speeds than at high speeds. In turning a corner at 15 or 20 miles per hr they will often develop a friction of 0.25 and 0.30, whereas at higher speeds, when the vehicle begins to "get light on its feet," less and less friction will be utilized as the speed is increased. In a report[16] to the Highway Research Board, Joseph Barnett, M. Am. Soc. C. E., reached the tentative conclusion that friction factors could be utilized in the design of horizontal curves on the basis of $f = 0.16$ up to and including 60 miles per hr with a reduction of 0.01 for each increase in speed of 5 miles per hr for values above 60 miles per hour. This is tabulated as follows:

Speed, in miles per hr	Friction factor "f"	Speed, in miles per hr	Friction factor "f"
60	0.16	85	0.11
65	0.15	90	0.10
70	0.14	95	0.09
75	0.13	100	0.08
80	0.12		

Mr. Barnett added that insufficient tests were available to confirm the trend for speeds greater than 60 miles per hr. Another factor, which the writer feels has an influence on the problem, is the difference in position of the center of gravity of the driver and that of the vehicle. It is believed that this factor will induce drivers to develop higher friction factors on highly superelevated curves than they will on curves with a low rate of superelevation.

In view of the conditions outlined herein it is felt that more tests and confirming evidence are required before authoritative rules can be advanced for curve superelevation.

[16] "Report of Project Committee on Relation of Curvature to Speed," by Joseph Barnett, Highway Research Board, November, 1936.

The writer feels that 10-ft lanes are somewhat narrow on a crowded structure carrying heavy mixed traffic. In the case of two maximum-width trucks each traveling, side by side, in the center of its 10-ft lane, the clearance between the trucks will amount to only 2 ft. Mr. Curtin is quite correct in stating that additional width should be provided in lanes next to curbs. Observations on the West Side Highway in New York City indicate that a large proportion of motorists operate from 4 to 6 ft clear of the right-hand curb. At a considerable increase in cost, lanes 10.75 ft wide were provided in the Lincoln Tunnel, and 11.5-ft lanes have been designed for the seven tunnels on the Pennsylvania Turnpike. In order to obtain the maximum efficiency from a crowded roadway it is essential that the lane lines be properly delineated with pavement markings.

The writer wishes to emphasize all Mr. Curtin has stated with respect to wye junctions at connections and to stress the undesirability of locating junctions close together. Perhaps nothing tends to confuse the motorist more or is the cause of more turbulence than a bewildering maze of junctions close together.

The desirability of providing tangent alinement at tunnel portals should be emphasized. On the Pennsylvania Turnpike particular attention was paid to this feature and tangents were projected out from the side of the mountain in the open for a distance of 800 to 1,000 ft from the portals in all but three of the fourteen approaches. In the event that curves are required within tunnels it is most desirable for tangent alinement to continue into the tunnel for a distance of not less than 600 ft (preferably 1,000 ft) before starting the curve. This will permit the eye of the driver to become accustomed to the change in light and to be able to see the curve well in advance.

The retinal adaptation of the eye at the entrance to tunnels was given careful consideration during the design of the Lincoln Tunnel and both portals were flared vertically to provide a gradual light change from daylight to the artificial illumination within the tunnel. It is believed that the flare at the New Jersey portal has produced satisfactory results. In New York, however, the shallow clearance beneath Tenth Avenue and the presence of numerous underground utilities prevented a sufficient amount of flare to accomplish completely satisfactory results. The tunnels on the Pennsylvania Turnpike have been provided with a vertical flare equivalent to approximately 29% of the normal ceiling height and with a modicum of auxiliary artificial illumination at the portals. Auxiliary lighting can be wired to separate circuits so that it may be turned off during the hours of darkness, thus relieving the reverse effect of blindness at night when leaving a tunnel. It would appear essential, however, that illumination be provided on tunnel approaches, decreasing in intensity as the distance from the portal increases in order to prevent blindness at night when emerging from a normally illuminated tunnel.

Pedestrian crossings are desirable not only from the standpoint of safety, but also to facilitate the movement of traffic. Irregular groups of pedestrians straggling across a plaza disrupt vehicular movement. When pedestrian travel reaches such proportions that officers must be stationed to halt vehicles for

pedestrian passage, the interruption to traffic assumes serious economic proportions when a large volume of vehicular traffic is involved.

In the design of grade-separated pedestrian crossings, the reluctance of the public to using stairways must be recognized and, when possible, inclined ramps with grades not exceeding 10% are desirable. Where steps must be used, fences or barriers should be constructed to prevent pedestrians from crossing the vehicular roadways; otherwise many persons will avoid the steps and continue to walk in the roadway. In considering a choice between an overhead and underground crossing it is well to weigh carefully the advantages and disadvantages of each type before a selection is made. The overhead type will require approximately twice as many steps as an underground passageway because of the required 14-ft vehicular clearance; but it is less likely to be a lurking place for criminals, and will require less supervision. The underground passage requires less steps but, on the other hand, it must be waterproofed, drained, adequately illuminated, and policed during periods when usage is light in order to prevent nuisances and the commission of crime.

In the past design of vehicular crossings there has been a regrettable lack of planning for bus stops adjacent to plazas and, as Mr. Curtin states, this has been the cause of inconvenience, traffic delay, and accident hazard. Each location is a specific problem. Some plazas may never have any appreciable bus-station traffic whereas others, as is illustrated by the George Washington Bridge, will become a concentration depot for bus passengers almost overnight. The author's suggestion of providing, initially, a planned space within which a bus station can be constructed if and when the need arises is the sensible solution to the problem.

In the design of any motor traffic facility, the needs and characteristics of the motor car and the limitations and weaknesses of the driver are fundamental considerations applicable to the general conception and to the design of each individual detail in order that the completed project may be as ideally suited to the use which will be made of it as possible. Mr. Curtin has constantly kept this objective before him in the presentation and has made a valuable contribution to engineering literature.

AMERICAN SOCIETY OF CIVIL ENGINEERS

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DISCUSSIONS

TREND IN HYDRAULIC TURBINE PRACTICE
A SYMPOSIUM

Discussion

BY MESSRS. MARTIN A. MASON, AND E. SHAW COLE

MARTIN A. MASON,[27] ASSOC. M. AM. SOC. C. E. (by letter).[27a]—The consideration of trends in hydraulic turbine practice which has been initiated by Messrs. Winter and Davis opens two related fields of discussion which should have an important place in the Symposium.

The importance of model tests in any turbine testing regimen is evident, and such tests admittedly are very useful; but the writer ventures the thought that their usefulness may be rather more limited than recent practice would indicate. Reference is made, of course, to the well-established fact that true similarity of model and prototype is not obtained, requiring, therefore, corrections to be applied to the transfer, or step-up, equations. This fact, in itself, casts considerable doubt upon the value of the model test results when scaled up to the prototype; and when the history of the correction formulas now in use is reviewed there is a tendency to conclude that the present method of predicting prototype results from model tests is open to objection.

It is true that supposedly "theoretical" analyses have resulted in published correction equations to be applied to the efficiency; but the results obtained by the different equations vary among themselves by an amount equal to 10% or more of the step-up they define. Moreover, a critical review of the derivation of these "theoretical" corrections shows that they may not be based on sound assumptions. Specifically, the assumptions regarding similar roughness in model and prototype appear to be questionable, as well as the use of Blasius' formula, which is known to be applicable only to Reynolds numbers of about 100,000 or less.

It would seem more reasonable to base the efficiency correction on experience curves relating model and field tests, rather than, in a sense, on artificial relations which may have little basis in fact. This could be done very simply

NOTE.—This Symposium was published in November, 1939, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: January, 1940, by Messrs. W. S. Pardoe, and Donald H. Mattern; and March, 1940, by Messrs. Lewis F. Moody, and R. E. B. Sharp.

[27] Asst. Hydr. Engr., National Hydr. Laboratory, National Bureau of Standards, Washington, D. C.

[27a] Received by the Secretary February 19, 1940.

by turbine manufacturers who, alone, have available the requisite information to define not only the efficiency relations but also the discharge and power relations. For the larger low-head turbines this procedure may not be possible since field-test information for most of the recently installed larger turbines of this type is not available. However, the adoption of a policy of field testing for acceptance in the future, in lieu of acceptance based on model tests, would soon provide sufficient data to allow the formulation of empirical corrections for this class of turbines, or the verification of the formulas used at present. This suggested policy leads to a discussion of field tests of hydraulic turbines.

Much has been written, pro and con, about the various measurement methods cited by Mr. Davis, and voluminous data have been assembled to prove the accuracy of this or that method, with the result that perhaps many engineers have become confused in the tide of claims, counterclaims, information, and misinformation. It might be worth while, therefore, to attempt to recall certain fundamental ideas pertinent to the water-measurement problem.

Each of the numerous methods of measurement that have been developed has some advantage that may or may not be common to other methods; and it is certain that the proponents of each method have satisfied themselves as to its advantages before lending their support to its exploitation. In this respect it is of interest to note in the Test Code of the Société Hydrotechnique de France the statement[28] that "even if none of these methods is perfect and universally applicable, there are, nonetheless, very few which are not of value when correctly used."

The writer feels that the poor results obtained in many applications of some of the measurement methods can be attributed to a lack of knowledge, on the part of the operators, of the conditions required for the correct application of the methods; that is, the methods were not "correctly used." A mere similarity of conditions of test is not sufficient to assure correct utilization of a method; it is rather of primary importance that the fundamental principles of the method be understood thoroughly and the proposed application of the method be examined with respect to these principles. In addition, a wide experience with the method seems to be necessary, particularly when the method involved is one of those whose fundamental principles have not been thoroughly delineated. With the knowledge and experience thus defined, one is in a position to hazard a prediction as to the accuracy of a particular measurement under given conditions, whereas without such a background little may be said regarding accuracy.

In the case of the measurement of water in low-head turbine installations, it seems that, with few exceptions, the profession has lost sight of these conditions. There has been little opportunity for any one to develop an appreciable background of experience, and no exhaustive studies have been made of the applicability of accepted methods of measurement to the particular conditions existing in low-head installations. At least three methods have been applied with more or less satisfactory results to low-head installations, only one of which, the two-type current-meter method, was developed with the operating conditions peculiar to low-head installations in mind; yet, this case

[28] "Code d'Essais des Installations Hydrauliques," 1re ed., *Société Hydrotechnique de France*, pp. 46, 47.

is admittedly one of the most difficult of water-measurement problems, even when ordinary accuracy is desired. One may well question the ability of any method not primarily designed for operation under the conditions obtaining in low-head installations to give results whose accuracy would consistently approach a fraction of 1%. In the interest of future application, and in view of the difficulties attendant on a more precise procedure, it would seem worth while to compare the various methods of measurement existent before arbitrarily attributing to certain of the methods advantages which they may not possess, or limits of accuracy which careful investigation and study may show to be unattainable. Until such information is available, logic would dictate the acceptance of the method specifically developed for application to low-head installations as being preferable to other methods whose suitability in this particular case has not been rigorously demonstrated.

In this connection the unsoundness of judging the accuracy of a measurement method by comparison with expected turbine efficiencies calculated from model tests must be evident. Professor Moody's remark in the paper presenting his formula for efficiency step-up—"Considering the fact that the losses in a turbine are dependent on many factors and involve many complications and that in comparing efficiencies only approximate results at best can be expected * * *" [29]—might well be recalled here.

As noted by Mr. Davis, the success of the Allen and Gibson methods is in part due to the strict control over their use exercised by Messrs. Allen and Gibson, who have found the thorough training of men in the use of their methods to be necessary. It is worth noting that in Europe neither of these methods is commonly accepted as a standard of measurement; the Test Code of the Société Hydrotechnique de France, for example, lists [28] them both in the class of methods whose "results appear uncertain either by reason of their principle of operation, insufficient development, or difficulties of practical application."

It appears that the profession needs to realize that, when high accuracy is required, water measurement is a specialized problem that may be handled successfully only by men having a background of knowledge and experience, using measurement methods whose applicability to the conditions of test has been proved. The observation of Mr. Davis regarding the necessity for trained engineers in the conduct of current-meter tests thus does not introduce a new requirement peculiar to this method of measurement, and the method should not be criticized on this account. In fact, there may be evidence of unsuccessful applications of accepted test code methods which would lend weight to the argument that these methods are at least as susceptible as the two-type current-meter method to difficulties traceable to inexperienced operators.

The unquestioned necessity for accurate and complete comparisons of model and prototype tests of turbine installations gives an increased importance to field testing of turbines. The consequent need for a clarification of the present unsatisfactory status of the water-measurement problem, particularly in regard to low-head installations, demands positive collaborative action on the part of both turbine manufacturers and purchasers or users. Comparative tests of

[29] "The Propeller Type Turbine," by Lewis F. Moody, *Transactions*, Am. Soc. C. E., Vol. 89 (1926), p. 626.

water-measurement methods to be made at the Santee-Cooper project on the suggestion of the Hydraulic Prime Movers Committee of the A. S. M. E. should be considered as only the first step in such a clarification.

E. Shaw Cole,[30] Jun. Am. Soc. C. E. (by letter)[30a]—Under the heading of "Field Tests" Mr. Davis mentions the various methods for measuring water and reviews the extensive research on the "multiple-current-meter method." It is the purpose of this discussion to bring attention to the equally extensive research on the pitot tube during the ten years 1929–1939.

Three papers since 1935 have presented the results of research at the Alden Hydraulic Laboratory and the University of Pennsylvania, on both the so-called simple and combined types of pitot tube. Two of these papers[31] were chiefly concerned with a combined type of tube that has been used successfully for accurate water measurements for more than forty years. Simultaneous measurements of turbine efficiencies using the pitometer and salt-velocity methods were presented and the close agreement was further proof that pitot tube measurements could be made with an error of less than 1%. The third paper[32] presented the results of a very thorough investigation of sources of error in pitot tubes. Various types of simple and combined tubes were studied, and one of the conclusions reached by Professor Hubbard was that the pitometer was a very convenient and accurate instrument.

Mr. Davis' statement, under the heading "Field Tests," that the pitot tube method becomes "unwieldy when the size of the conduit passes medium size and for medium or large volumes of water" is vague and somewhat misleading. All methods of water measurement have limitations. For the pitot tube the size of conduit and velocity of flow may introduce mechanical difficulties, but for most cases these can be overcome by proper design of the pitometer rod and means for handling it. Such measurements have been made without difficulty in pipes 12 ft in diameter where the velocity was 9 ft per sec, and in 6-ft pipes where the velocity was as high as 19 ft per sec. Regular present-day flow measurements in pipes and on ships cover a range of velocities from 0.7 to 50 ft per sec, and although an extreme combination of large diameter with high velocity could present difficulties this would be unusual. For most cases the advantages of the pitot tube method (low cost and speed) are available to the profession without a sacrifice of accuracy.

The writer heartily agrees that strict control of the various methods of water measurement is highly desirable and would like to emphasize that the pitot tube method also should be applied only by men trained and experienced in its use.

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[30] Engr., The Pitometer Co., New York, N. Y.

[30a] Received by the Secretary February 21, 1940.

[31] "Pitot-Tube Practice," by Edward S. Cole, M. Am. Soc. C. E., *Transactions*, A. S. M. E., Vol. 57, 1935, p. 281; also "Pitot Tubes in Large Pipes," by E. S. Cole and E. Shaw Cole, *loc. cit.*, August, 1939, p. 465.

[32] "Investigation of Errors of Pitot Tubes," by C. W. Hubbard, *Transactions*, A. S. M. E., August, 1939, p. 477.

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DISCUSSIONS

SEWAGE DISPOSAL PROJECT OF BUFFALO, NEW YORK

Discussion

By Messrs. Arthur J. Bulger, and C. R. Velzy

Arthur J. Bulger,[3] Esq. (by letter).[3a]—The author's purpose to record some of the engineering data accumulated during the design and construction of the project has been ably effected. The information is a valuable addition to the fund of such data.

A statement of the factors leading to the selection of the percentage of annual costs to be raised by a charge based on water consumption, and its relation to that portion of the annual costs to be derived from an ad valorem tax, would have been both interesting and timely. Payment of annual costs for sewerage works by a rental charge for services is not new, but its use has been limited until the past few years to a relatively small number of plants.

In principle the two-part rate schedule adopted by the Buffalo Sewer Authority appears to offer a more equitable distribution of cost to benefits than an all-revenue schedule or payment entirely by ad valorem tax. The issuance of part revenue bonds and part general obligation bonds as security for construction funds has resulted, in some cases, in a similar allocation of costs. However, in this procedure, operating expenses are usually paid entirely from revenues.

Digressing somewhat from the stated purpose of the author, the project at Buffalo is of peculiar interest because of its international aspect. Although a problem of the local community, the completion of the project should be gratifying to the nation since it has removed a condition displeasing not only to the citizens of Buffalo but also to the nation's good neighbors in the north as well. Furthermore, this project is an example of the influence of the Public Works Administration in materially accelerating the job of removing undue pollution from surface waters.

Note.—This paper by Samuel A. Greeley, M. Am. Soc. C. E., was published in October, 1939, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: November, 1939, by C. A. Holmquist, Esq.

[3] Director, Eng. Div., Federal Works Agency, Public Works Administration, Washington, D. C.

[3a] Received by the Secretary February 19, 1940.

The Buffalo Authority and its engineers are to be congratulated on a job well conceived and executed.

C. R. Velzy,[4] M. Am. Soc. C. E. (by letter).[4a]—Some of the operating results of the sewage treatment works at Buffalo are reported herein, with comments on the accomplishments thus far attained by the sewage disposal project.

The first sewage, amounting to about 20 mgd, was turned into the interceptor on June 18, 1938. Following that date, other connections from various parts of the Buffalo sewer system were opened into the interceptor until, by July 1, 1938, the entire flow of the city was being received, pumped, and treated at the treatment works. Although final construction was not completed for more than a year thereafter, the structures essential to the starting of treatment were ready for use on this date, and treatment has continued without interruption since that time.

Records of operating results have been kept from the beginning, some of the more pertinent being included in Table 9. Where available, corresponding data from the bases of design are shown. It should be borne in mind that, due to construction and testing activities during the first eighteen months of operation, the results do not, in all cases, represent normal conditions. Partial interruption of settling tank operation and return of heavy supernatant to the settling tanks during interruptions in sludge disposal processes are illustrative of interferences with normal operation.

Table 9(*a*) contains data on population and flows. It is worthy of note that the population has not increased as rapidly as was expected in 1935. The average flow has been very close to the rate estimated, but, as might be expected, maximum rates have varied somewhat from the estimate. The maximum rate of 560 mgd occurred on September 14, 1938, and lasted about one hour.

The data on grit and suspended solids shown in Table 9(*b*) indicate raw sewage characteristics in accordance with the bases of design. The removal of suspended solids was handicapped by necessary irregularity in plant operation. Results obtained in some of the more normal months indicate that the removal will range from 43% to 50%, with an effluent containing somewhat less than 100 ppm of suspended solids.

Although the chlorine demand has been consistent, there are wide variations in short-period demand due to return of heavy supernatant and also apparently to the occasional discharge of industrial wastes of high demand. By hourly sampling and testing, the application of chlorine is made to follow the requirements closely, and a satisfactory kill of coliform bacteria is maintained at all times as shown in Table 9(*c*).

The raw sludge has been characterized by low moisture content and a low percentage of volatile matter. The volatile content was affected by return of heavy supernatant which was low in volatile matter. Gas production has been good, and in October, 1939, the maximum rate of gas production was reached,

[4] Works Supt., Buffalo Sewer Authority, Buffalo, N. Y.

[4a] Received by the Secretary February 27, 1940.

amounting to 0.93 cu ft per capita per day. These data are shown in Table 9(*d*).

TABLE 9.—Operating Data, Sewage Disposal Project of Buffalo, N. Y.

Item	Description	Bases of design	July, 1938, to June, 1939	July, 1939, to January, 1940
	(*a*) Population and Flow Characteristics			
1	Population	640,000	600,000	600,000
	Flow:			
2	Average, in gallons per capita per 24 hr	200	217	205
3	Average, in million gallons per 24 hr	128	130	123
4	Maximum monthly average, in million gallons daily	200	170	133
5	Maximum daily flow, in million gallons	450	334	240
6	Maximum rate, in million gallons daily	570	560	397
	(*b*) Solids			
	Suspended Solids:			
7	In raw sewage, in parts per million	180	200	178
8	In pounds per capita per 24 hr	0.30	0.36	0.31
9	Percentage removal	48.0	41.0	37.1
10	Removal, in pounds per 24 hr	92,000	88,000	69,000
11	Grit, in cubic yards per million gallons	0.15	0.16	0.15
	(*c*) Chlorination			
12	Demand, in parts per million	....	5.82	6.27
13	Average use, in pounds per 24 hr	....	4,880	6,797
	Percentage Kill of Coliform Bacteria:			
14	Maximum monthly average	....	99.9	99.9
15	Minimum monthly average	....	94.2	97.2
16	Average	....	98.8	98.9
	(*d*) Sludge Characteristics and Gas			
	Percentage of:			
17	Moisture in raw sludge	96	96.6	92.6
18	Volatile matter in raw sludge	70	57.6	57.7
19	Moisture in digested sludge	....	87.8	89.5
20	Volatile matter in digested sludge	....	41.5	46.3
21	Gas production, in cubic feet per 24 hr	....	320,000	531,000
	(*e*) Sludge Disposal			
	Percentage of:			
22	Ferric chloride in dry solids	2 to 3	2.31	2.95
23	Lime (CaO) in dry solids	8 to 12	9.25	10.25
24	Moisture in sludge cake	70	61.8	62.3
25	Volatile matter in sludge cake	....	38.8	37.2
26	Filter rate, in pounds of dry solids per square foot per hour	5.0	....	8.3
27	Incineration: Dry solids in pounds per furnace hour	2,500	....	2,610

Some data on sludge disposal are given in Table 9(*e*). The filtering of sludge has been particularly successful in the low moisture obtained in the sludge cake and in the relatively high filtering rate. The rate for the last six months of 1939 was 8.3 lb of dry solids per sq ft per hr as compared to a guarantee of 5 lb. The maximum rate of 13.4 lb per sq ft per hr was reached in

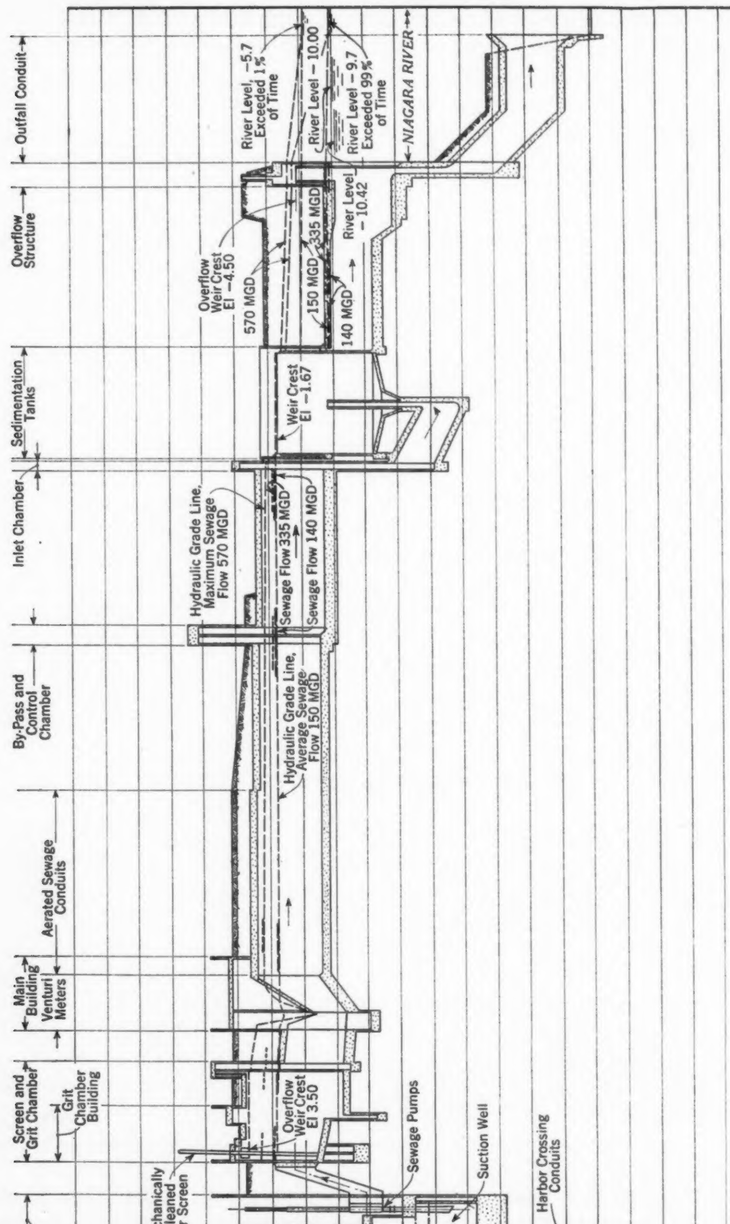

FIG. 8.—HYDRAULIC PROFILE; MARCH, 1938; SEWAGE TREATMENT PLANT

August, 1939. It is expected that refinement in the control of the application of chemicals will result in the use of less chemicals than have thus far been found necessary.

During the first year, the incinerators were under tests most of the time and rates of burning were not recorded. However, from July, 1939, to January, 1940, the rates of burning of dry solids have averaged 2,610 lb per furnace hour as compared to a guarantee of 2,500 lb per furnace hour.

Fig. 6 shows a profile through the treatment works and a high-water and low-water hydraulic grade line. Although no observations have been made to determine the rate of sewage flow at which spilling over the overflow weir in the outfall takes place, observations of the hydraulic grade line through the plant for a high and a low rate of flow are shown in Fig. 8.

Under the heading "General Statement" Mr. Greeley presents the three objectives of the sewage disposal project. Monthly observations of the bordering waterways have been made by the laboratory staff under the direction of George E. Symons who has charge of the laboratory work of the Buffalo Sewer Authority.

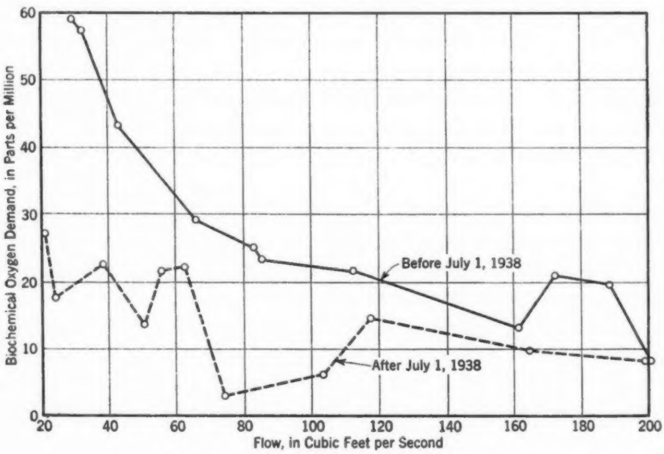

FIG. 9.—CONDITIONS IN BUFFALO RIVER

Results of observations on the conditions in the Buffalo River before the treatment works went into operation and after they were in operation are shown in Fig. 9. These data are from averages of analyses of samples taken at four sampling stations in the middle stretches of the river somewhat above the outlet. The data and results herein are based on flows as great as 200 cu ft per sec. It will be noted that the data taken after July 1, 1938, even for a lower average flow, show a definite decrease in 5-day B.O.D. Observations showed a reduction in coliform bacteria of 43%. A definite increase in dissolved oxygen is evident in that, before July, 1938, it was found only in flows greater than about 160 cu ft per sec, whereas, after that date, it was found in flows as low as 100 cu ft per sec.

Some parts of Buffalo Harbor are directly affected by the Buffalo River. These parts enjoyed an improvement corresponding to the improvement in the river. Before July, 1938, Black Rock Harbor became quite septic in the summer months, with a high bio-chemical oxygen demand and coliform content. This was due to the discharge of one of the main sewer outlets into the harbor. After the treatment works went into operation, the waters of this harbor became fresh, with a dissolved oxygen content of about 6 to 7 ppm in the summer months. The reduction in a 5-day B.O.D. is shown in Table 10.

TABLE 10.—COMPARISON OF BIO-CHEMICAL OXYGEN DEMAND, IN PARTS PER MILLION

Date (1938)	January	February	March	April	May	June	July	August	September	October	November	December
Before July 1	28	15	3	30	35	29	17.	11	24	28	..	33
After July 1	3	..	..	5	0	4	0.2	1	2	2	2	2

Monthly samples have been taken and analyses made on the Niagara River, and it has been observed that the quantities of coliform bacteria have been reduced about 97% from the average for some two years previous to the beginning of operation. Water works below Buffalo report longer filter runs and reduction in the quantity of wash water and chlorine required. Reduction in sludge deposits and the return of game fish immediately below Buffalo are reported.

By intercepting Scajaquada Creek the Main Intercepting Sewer has removed a flow of sewage from Park Lake, an attractive body of water in Delaware Park, and has thus eliminated a serious odor nuisance which previously prevailed during the summer months.

With the completion of construction and testing, normal operating results as well as refinements and economies are anticipated. Further work on industrial waste disposal, with the continuation of excellent cooperation from the industries involved, will result in further improvement in the condition of adjacent waterways.

The operating results at the treatment works and the effects upon the waterways of Buffalo obtained during these first months of operation indicate the soundness of the bases of design and show accomplishment of the objectives which are described in Mr. Greeley's paper.

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DISCUSSIONS

TRANSIENT FLOOD PEAKS

Discussion

By Messrs. James M. Fox, F. C. Finkle, A. L. Sonderegger, and Harold C. Troxell and R. Stanley Lord

James M. Fox,[22] Jun. Am. Soc. C. E. (by letter).[22a]—The data of the notable New Year's floods of December 31, 1933, to January 1, 1934, in Southern California are utilized by Mr. Lynch in a manner quite different from that of other investigators in the same field. The conception of a momentary peak discharge far in excess of that which could be sustained by the maximum rate of precipitation should be of great interest to highway, railroad, and flood-control engineers. The most important lesson to be derived from the paper is that of anticipating such phenomena in the design of structures intersecting the channels of canyons such as those described by the author. In this respect, knowledge of the cross-sectional area occupied by the surge is far more essential than knowledge of the velocity or volume of the discharge. However, in neglecting to consider, fully, the effects of the heavy debris content on the formation of the flood peak, the author overlooked an opportunity to augment greatly the scope and interest of his paper.

As long as runoff and discharge are steady, the discharge rate remains at some value below the rate of precipitation. Obviously, in order to have a temporary peak created, in which the discharge is greater than that which could be sustained by the maximum rainfall intensity, a break in stream velocity must occur, by means of which slower moving water is overtaken by more rapidly flowing water. It does not appear invariably necessary, however, that this should be caused by a sudden increase in the rate of upstream runoff. The effect of an excessive peak could be produced as readily by a retardation of downstream velocities.

The statement is made that the debris content of the floods was from 50 to 70%. It is further stated that surface velocities were from 25 to 30 ft per sec, although the author confesses a lack of knowledge as to the velocities

Note.—This paper by Henry B. Lynch, M. Am. Soc. C. E., was published in November, 1939, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: January, 1940, by Ivan E. Houk, M. Am. Soc. C. E.; and March, 1940, by Messrs. Gordon R. Williams, and Donald M. Baker.

[22] With U. S. Geological Survey, Salt Lake City, Utah.

[22a] Received by the Secretary February 19, 1940.

within the flow itself. Probably a mean velocity of 10 ft per sec across the section is not too low. If this were the case, the average peak discharge in the zones classified by the author under (*a*), (*b*), and (*c*), in Table 1, would have been 6,890, 2,830, and 1,740 cu ft per sec per sq mile, respectively, assuming only free water flowing. If one half of the areas were occupied by solid matter, however, the water runoff rate per square mile would then become 3,445, 1,415, and 870 cu ft per sec per sq mile. These values, although far more conservative than those obtained by using the author's velocities and cross-sectional areas as signifying the discharge of water only, are still large enough to lend support to the conception of excessive peak discharge. Apparently, the author took precaution, in his section measurements, to eliminate the effect of stream-bed erosion. It is presumed that these precautions were applied to care for possible increases in width, as well as depth, due to scouring.

In view of the large debris content of the floods, the writer is disposed to believe that excess cross-sectional area of the stream resulted from the velocity of the surge being greatly retarded, permitting the less-laden upstream runoff to overtake and add to the volume of the leading wave. All evidence available on the behavior of debris flows indicates that the surge travels with a "rolling" motion and with a high degree of turbulence. The corrosion of the channel is accomplished more by the violent vertical currents within the mass than by any great horizontal velocities. As fresh material from the channel is acquired by the fore part of the flow, tending to thicken the mass and retard its velocity, freer water from upstream, overriding the heavily loaded portion, overtakes and merges with the head of the surge, tending to reliquify it. This action may easily be simulated in miniature in the laboratory or in the field.

Mr. Lynch refers to the great floods of Northern Utah in 1923 and 1930. In the interest of advancing the understanding of these phenomena, the writer is impelled to enlarge somewhat upon the data supplied by the author, which, it is inferred, were obtained entirely from the references cited.

The writer has spent a year (1938–1939) on an investigation of cloudburst floods in Utah, under the direction of Ralf R. Woolley, M. Am. Soc. C. E., senior hydraulic engineer with the U. S. Geological Survey, at Salt Lake City. Three months of this period were passed in field exploration of some of the canyons involved in such floods, some of which are mentioned by the author. The remainder of the time was spent in the office on miscellaneous studies of these floods.

The storms producing the floods referred to by the author were entirely dissimilar to those causing the California New Year's disaster. No rain had fallen in the northern Utah areas for several days prior to the storms, and when they struck nearly all of the precipitation fell within the space of an hour.

The surges, or debris flows, accompanying these storms deposited on the fertile fans at the canyon mouths volumes of debris estimated at from 50,000 to 80,000 cu yd per sq mile of catchment area, practically all of which was removed from the channels themselves. In most cases there was but little runoff in advance of the great crest of the debris-charged flood. In some in-

stances the material moving across the fans was so thick that its margins came to rest in banks 2 to 4 ft high, and with slopes of 30° to 40°, as if placed by a bulldozer.

Unfortunately, no accurate data could be uncovered as to the velocity or cross-sectional area of the flows. The streams explored by the writer had experienced two or more such floods in the 15 years since 1923. High-water marks had been obliterated by extensive undercutting and slumping of the high cut banks in the alluvium of the canyon bottom. In all probability the precipitation in the higher areas attained a rate of more than 4 in. per hr, for the space of a few minutes. It is possible that peak discharges in the main channel exceeded the maximum rates of surface runoff into the channel system. The effect of the high intensity of rainfall for the single, brief period was modified materially, no doubt, in the transit of the surface runoff down the mountain slopes and by absorption. However, the peaks in the main channel were the result of the accumulation of water from upstream behind what might be termed "moving dams" of debris-choked, muddy water.

It is stated by the author that the storm of August 13, 1923, in Northern Utah yielded less than 1.5 in. of rain. So far as has been found, the only direct recordings of rainfall in the area were obtained at stations of the U. S. Weather Bureau located near the foot of the western slope of the Wasatch Mountains, against which the storms impinged, and which are drained by the flooded canyons. These measurements showed total precipitation depths of from 0.64 to 1.34 in., depending upon their distance from the mountains and their location with respect to the storm center. The gages were all below an elevation of 4,500 ft, whereas the elevations of the catchment basins of the flood canyons ranged from 4,500 ft at the mouths to from 8,000 to 9,000 ft at the divide, 3 or 4 miles farther to the east.

Early in 1939 the writer made a rather extensive analysis of the variation of rainfall with altitude on the western slopes of the Wasatch Mountains, paying particular attention to the more violent summer thunder-storms. Based on the results of these studies, it is estimated that the depth of rainfall during the storms over the flood canyons varied from 1.25 to 2.50 in., depending upon the elevation, and averaged greater than 2.00 in. in depth over the entire area.

This greater volume of rainfall is confirmed also by the quantities of debris removed by the floods. Besides the writer's estimates, made in 1938, one of the references[6] cited by Mr. Lynch stated that in 1923, at Willard, Utah, 155 acres of land were covered by from 1 to 3 ft of rocks and earth. The minimum depth gives a total volume of 250,000 cu yd, or 62,500 cu yd from each of the 4 sq miles of the Willard Creek watershed. This is equivalent to three quarters of an inch in depth from the entire surface area. From several porosity determinations and shrinkage measurements it was found that at least an equal volume of water would have been required to liquify the debris mass, assuming that no water ran off beyond what was required to saturate the flow.

[6] "The Floods of 1923 in Northern Utah," by J. H. Paul and F. S. Baker, *Bulletin* of the Univ. of Utah, Vol. XV, No. 3, March, 1925.

A glance at Fig. 9 will illustrate the improbability of a rain of less than 1.50 in., falling as recorded, accomplishing that amount of transportation. The intensities shown are for even 5-min intervals. The actual measured maximum intensity was 4.2 in. per hr (0.35 in. between 6:53 p.m. and 6:58 p.m.). The solid lines represent the intensities of the storm of August 13, 1923, as recorded by the recording rain gage of the U. S. Weather Bureau at Salt Lake City. Fig. 9 shows that if such a storm fell on ground whose dry infiltration rate was approximately 1 in. per hr, only about 0.5 in. would have been available for runoff.

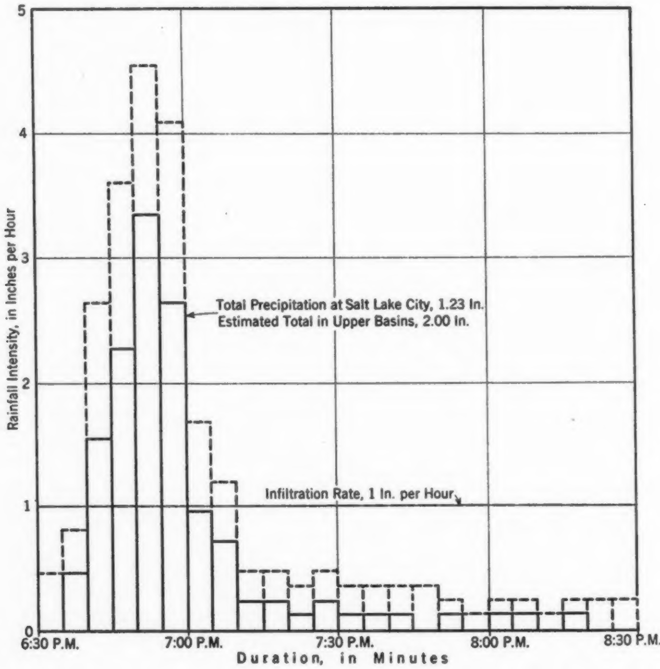

FIG. 9.—STORM OF AUGUST 13, 1923, AS RECORDED AT SALT LAKE CITY, UTAH

The dotted outline (the average precipitation over the basins, August 13, 1923) is based upon the writer's estimate of 2 in. in depth. It was assumed that both duration and intensities were increased somewhat by the effect of the rising terrain over which the storm was passing. If the rain fell in a manner similar to that indicated, and an infiltration rate of 1 in. per hr is again assumed, about 1 in. would have run off in the course of half an hour. How much more of the rain would have been absorbed in transit over the slopes is a matter of conjecture. Moreover, if the various determinations of debris volumes are reasonably correct, the writer's estimates of rainfall and intensities are more likely to be low than high.

F. C. Finkle,[23] Esq. (by letter).[23a]—Before discussing the author's hypothesis, it should be clearly understood that the type of floods to which it is applicable is not the same as that resulting from what are called "cloudbursts" in the Western States. Cloudbursts are not preceded by any rainfall that can be regarded as initial. On the contrary, the ground is dry before precipitation begins; the downpour is most rapid at the start and, after exhausting itself within a short space of time, slackens to a slow drizzle of short duration, or stops abruptly. Although little has been learned concerning the rate of precipitation causing cloudbursts, some facts have been recorded indicating rates varying from 10 to 20 in. per hr, but rarely, if ever, lasting for nearly one hour. The writer observed an instance in the Crafton Hills near Redlands, Calif., during August, 1891, in which 8 in. of water fell in 37 min, starting without any previous fall and ending without tapering off perceptibly. He also observed another during the summer of 1903 in a small canyon east of Mojave, Calif., in which 5.5 in. fell in 22 min.

These and similar downpours on a dry surface result in practically a 100% runoff in a short time and produce a nearly vertical wall of onrushing water in the drainage channel. These torrents are entirely consistent with the total fall of water within a given time, because there is even less absorption than the volume of mud and debris picked up and carried by the water, which compensates for the penetration loss. The cloudburst falling on a hot, dry, and dusty surface causes the first globules of water to become encased in a film akin to the spheroidal film around a drop of water thrown upon a hot stove, thus preventing any considerable penetration. The rapidly descending volume is also retarded by debris and surface irregularities, until these are absorbed into the water. In this manner, both lack of penetration and checking of flow in the early stage of downpour result in piling up the traditional wall of water.

The so-called Montrose flood near Glendale, Calif., which reached its peak near midnight of December 31, 1933, and January 1, 1934, was in no manner comparable to a western cloudburst. It was the culmination of a sustained foothill and mountain rainfall commencing on December 30. It was the second most severe downpour and flood witnessed by the writer in this same locality, being surpassed only by the previous one of December 24–25, 1889, at which time there were no devices for measuring precipitation intensities at any point in Southern California. However, the much greater volume of debris in the earlier flood, and the greater distances to which it was carried, as well as the higher flood peaks, in spite of the less impervious character of the watershed surface, all indicate that flood to have resulted from an intensive precipitation more than twice as great in a single hour as the highest for December, 1933. The only data available on the 1889 intensity are two rough observations made near Westlake Park in Los Angeles for the hour from midnight of December 24 to 1:00 a.m. of December 25, 1889, in tin cans, one by the writer showing 2.89 in. and the other by Burr Bassell, showing 2.75 in.

During the 1933–1934 Montrose flood, the writer resided in the City of Burbank, Calif., which is a short distance from the foot of the Verdugo Moun-

[23] Cons. Engr. and Geologist, Burbank, Calif.

[23a] Received by the Secretary February 21, 1940.

tains, southerly from Stough Canyon and southwesterly from Sunset Canyon in the Verdugo Mountains. He spent both days, December 31, 1933, and January 1, 1934, at home because of the heavy downpour and dangerous condition of the streets. He made several trips, during the afternoon of December 31 and up to 10:00 p.m., as far east as the Glendale or Verdugo wash, observing the constantly rising flood water, after which all the streams from Stough Canyon to Verdugo wash became impassable.

No professional visit to the area covered by the Montrose flood was made by the writer until January 30, when engineering observations and studies were made from Haines Canyon on the west to Hall-Beckley Canyon on the east. The Pickens Canyon data were the only ones fully worked up and analyzed by the writer. The best section found in this canyon was near the Gould Castle with a good stretch of uniform channel, not affected by remnants of partly destroyed check dams, and with but shallow deposition after the flood peak. The hydraulic properties of this stretch were as follows: $a = 572.9$ sq ft; $p = 71$ ft; $r = 8.07$ ft; and $s = 0.09$. Data, therefore, were secured from cloudburst floods, saturated with silt, sand, gravel, and other debris passing over the diverting dam at Mill Creek No. 3 hydroelectric plant, near Forest Home, Calif., where accurate gagings of summer cloudburst floods had been made. On the basis of these data the value of n in Kutter's formula was placed at 0.07 for calculating discharge, which gave 16,267 cu ft per sec.

This peak flow was far from being water that had descended on the 1.78 sq miles of mountain drainage above the station. The remnants or locations, where they had existed, of more than 500 check dams, which had been constructed by the Los Angeles County Flood Control District of loose rocks and gravel wrapped in wire netting, were examined, and their contents in the washed out sections and storage behind them were measured and calculated. This volume of about 3,300,000 cu ft, about one half water and one half silt, sand, gravel, and rocks was added to the peak flood.

Next, by means of standard formulas applicable to a small drainage such as Pickens Canyon, it was determined that, with the rainfall coming as shown by the Los Angeles County Flood Control rain gages, the peak flood should have been about 5,300 cu ft per sec in water, taking into account the manner of concentration and the pyramiding of the runoff from the intensive period upon the channel flow preceding. Without considering the debris gathered by this 5,300 peak, there were nearly 11,000 cu ft per sec of the measured peak to account for. This differential must be accounted for by the water and debris from the failed check dams, other debris picked up by moving water on the slopes and in the channels, or on the theory advanced by the author.

Obviously, the volume in cubic feet per second resulting from the obliterated check dams would depend upon the duration of the peak, after reaching its maximum and before it began its rapid decline. The only data on duration of maximum peak came from those who saw the flood from the banks of Pickens Canyon at points below Gould Castle. The few critical observers interviewed differed widely, ranging from 5 min to about a quarter of an hour. From these statements, the writer reached the conclusion that the sustained maximum peak did not exceed 9 min, which would cause the water and debris from the check

dams to have added 6,000 cu ft per sec to the peak flood, on the assumption that the check dams failed during the time of peak. The writer does not wish to force any of his own opinions on others, but desires to say that he reached the firm conclusion that the check dams went out with the first onrush of the peak flow near the head of the canyons, and were washed out one after the other as the peak traveled downstream. If it happened in this manner, not only would 6,000 cu ft per sec of water and debris be added to the volume, but the momentary delay, as one check dam after another was hit, would accumulate volume sufficient to compensate for channel storage and to raise the peak flow progressively.

If the foregoing solutions are accepted (as they are by the writer), there are less than 5,000 cu ft per sec of the measured peak to be accounted for, which must have consisted of solid material such as ashes, silt, sand, gravel, rocks, and boulders from the slopes and channels; or, from the author's hypothesis, resulted from hydraulic surges in the channel flow itself, as illustrated in Fig. 1. If the debris content was about 50% of the volume, as pointed out by the author under "Debris Content," and writer's solution, as given herein, explains the great volume of the peaks; and the surge theory is unnecessary to account for runoff in excess of rainfall, as the solid content is computed to be less than 50%.

That channel surges occur in flood channels during protracted rain storms is a well-established fact. They may result from an "hydraulic jump" in the channel, in which case their effect is slight, or from a marked increase in the rate or intensity of precipitation during a storm, as pointed out by the author, in which case the effect is violent, because the resulting higher channel velocities cause the later discharge to overtake the lesser previous one. Surges also result from overloading a stream with debris, resulting in a pause of varying duration, in which case the stream volume may become greatly augmented by reason of the interruption to its uniform progress.

In the Montrose flood, under discussion herein, the increase in rainfall intensities was a minor factor in causing surges and radical augmentation of stream volume. In fact, as the author remarks (see heading "Verdugo Mountains: Rainfall on January 1, 1934): "There were no noteworthy short-time rainfall intensities." Thus, the cause being nearly absent, the effect was absent also; but other causes were present, which accounted for the noteworthy surge phenomena in that flood, and are stated in the order of their importance, as follows: (1) The great number of flimsily built check dams, which failed during the flood; (2) the loosened rock material and ashes caused by recent burning over of the drainage areas; and (3) debris collected from the bottom of the stream channels, due to the numerous small waterfalls over the check dams before they failed; these disrupted the stream beds and changed their regimen.

Although the author ignores the effect of the check dams, he states (see "Flood of January 1, 1934"): "The greatest surges were those in the then recently burned area." This statement agrees with observed fact, but is incomplete without including the check dams in channels within the then recently burned area.

Other studies made by the writer in Haines, Blanchard, Dunsmuir, and Hall-Beckley canyons, etc., bear out the foregoing statements, so far as the

data have been analyzed; but these analyses do not go beyond computing the peak volumes, and detailed data on other matters are lacking. A study was also made of the peak in Verdugo Canyon, where all the streams from the Montrose-Crescenta area concentrate, but the results do not bear on the question considered herein, because of channel and flood-plane regulation and varying time for synchronizing the flows below the foot of the Sierra Madre.

A. L. SONDEREGGER,[24] M. AM. SOC. C. E. (by letter).[24a]—Large areas of the choice foothill lands of Southern California occupy the coalescent debris cones of streams which issue from high mountain ranges. A clear conception of the phenomena which accompany the torrential floods of these streams is essential to the planning of flood protection works. Mr. Lynch's paper contributes information that is valuable to the analysis of the problems presenting themselves to flood control engineers.

Apparently, the conclusion to be drawn from the paper is that "floods of the so-called 'cloudburst' type yield momentary runoff peaks entirely out of proportion to the rate of rainfall." Since this conclusion is principally based upon data and records pertaining to the storm of December 31, 1933, in La Crescenta and La Cañada valleys, a foothill area near Pasadena, it may be appropriate to review some of the conditions surrounding this event. This was an important flood in that it led to the adoption, in Los Angeles County, of debris basins for the control of the floods of minor steep watersheds, and to the abandonment of loose rock wire-wrapped check dams as a major flood control measure.

Reference to the flood of January 1, 1934, in the same locality, was made by the writer in 1935.[25]

The individual watersheds contributary to the area are small—less than 2 sq miles. The mountainous sections consist of relatively steep slopes, covered with a veneer of residual granitic soils, generally 1 to 3 ft in depth. Creek channels are deep incisions which, in the long-period intervals between heavy storms, accumulate large quantities of debris, but are scoured clean during major floods. Most of the creeks are entrenched in an ancient alluvial mesa extending from mouths of canyons to the debris cone. The disaster affected the settlements on the modern cone area. A major portion of the watershed was burned in November, 1933. As a measure of protection to the residential settlements, the channels of creeks were stepped down by the construction of check dams, beginning at the lower mountainous section and extending on to the debris cone as far as the State Highway. These checks were wire-wrapped boulder rolls about 4 ft high and 150 ft or more apart.

No major flood had occurred in this area for about 17 years and creek channels showed a corresponding effect of an accumulation of debris. Moreover, the check dams had formed a succession of small basins which would intercept debris rolling in from steep slopes, or carried by the streams. There-

[24] Cons. Engr., Los Angeles, Calif.

[24a] Received by the Secretary February 26, 1940.

[25] "Modifying the Physiographical Balance by Conservation Measures," by A. L. Sonderegger, *Transactions*, Am. Soc. C. E., Vol. 100 (1935), p. 284; also "Flood in La Cañada Valley, California," by H. C. Troxell, Assoc. M. Am. Soc. C. E., and John Q. Peterson, U. S. Geological Survey *Water Supply Paper No. 796–C*, 1937.

fore, the "stage was set" for sheet floods from steep burned watersheds, a preliminary storage of debris of large proportions behind the checks, and a subsequent removal of detritus, new and old, when runoff had assumed such proportions as would set the alluvial mass into motion.

The major residential settlement of the area of coalescent cones occurred during the period from 1917 to 1933. The disaster occurred on January 1, between 12:00 and 12:30 a.m.

The accumulated rainfall for the storm of December 31, 4 a.m. to 12 noon, is as follows:

Area	Rainfall, in inches	Elevation, in feet
Flintridge	10.5	1,325
Sister Elsie Peak	7.8	4,950
Haines Canyon	9.0±	3,470
Pasadena	9.65	865

The greatest rise in 5 min intensity at Sister Elsie Peak was at 11:40 p.m. from a rate of 0.60 to 1.56 in.

The maximum intensities in the valley station of Flintridge were as shown in Table 5(*a*). These are not really excessive rates and are far below cloudburst proportions. For example, in the heavy rain of January 8, 1940, the Upper Haines gage at the westerly margin of La Crescenta Valley furnished the record given in Table 5(*b*). The conclusion reached is that the major cause of the disaster was the saturation of the denuded slopes by an accumulated

TABLE 5.—COMPARISON OF MAXIMUM RAINFALL INTENSITIES

Time period, in minutes	(*a*) FLINTRIDGE FIRE STATION, DECEMBER 31, 1933		(*b*) UPPER HAINES GAGE, JANUARY 8, 1940	
	Rainfall, in inches	Intensity, in inches per hour	Rainfall, in inches	Intensity, in inches per hour
4	....	....	0.62	9.30
5	0.24	2.88	....	....
10	0.34	2.04	0.80	4.80
20	0.60	1.80	1.00	3.00
30	0.84	1.68	1.27	2.54
60	1.33	1.33	1.37	1.37
120	2.14	1.07	....	....

rain of 9 in. within a period of 20 hr, and a final increase in intensity from 0.60 to 1.56 in. for a 10-min. duration.

Although an accumulated rain of 9 in. exceeds that normally occurring during capital floods in this area, the rate of maximum intensity has frequently been greatly exceeded. The conclusion is that other combinations of accumulated rain and intensities on similarly located burned-over watersheds are conceivable, which may produce more severe flood conditions.

Peak Flows.—Certain facts and conclusions become apparent from a study of Table 1:

1. Cross sectional areas of peak flows of recently (November, 1933) burned-over slopes average 689 sq ft per sq mile as compared with 283 sq ft of slopes

burned in 1927, and 174 sq ft of unburned small canyon watersheds. In estimating probable flood peak flows, the conditions imposed by a recent burn, therefore, should form the basis of assumptions.

2. The cross-sectional areas of peak flows per square mile of burned watersheds show widely different values. Since the factors that affect runoff, such as the state of the watershed cover, slopes, orographic conditions, and rainfall, were very much alike in the adjacent burned-over drainages and would call for more uniformity in wetted channel sections, it is apparent that causes other than those normally present were responsible for such erratic values.

Abnormal conditions may have resulted from sheet floods, landslides, log and boulder jams, failure of check dams, and scour.

The existence of check dams would result in a high-water mark during the first stage of the flood flow. Checks would act as debris barriers and would become completely backfilled. After the collapse of checks, an enormous viscuous mass of debris would be subject to removal, resulting in a deep scour of the creek beds and new high-water marks, but at lower elevations. The resulting cross sections, measured after the flood, therefore, would be representative of peak flows at two different stages, the high-water mark resulting from the earlier peak and the scoured bottom from the later one. The inference is that these cross sections, combined, gave values too large and not representative of any stage of the flood; the corollary is that runoff estimates based thereon would be misleading.

The composition of the fluvial mass removed during the flood was apparent from deposits above the abutments of check dams. It was a heterogeneous mass of unassorted material resembling, in composition, the well-known mud flows caused by cloudbursts in desert areas.

The numerous scars visible on the mountain slopes after the flood pointed to land slides of saturated residual soil cover, shoestring and gully formation, and bare areas, were proof of sheet floods carrying a heavy percentage of silt and debris at the time of peak flow. Indications are that the bulking of the viscuous mass may have been in the proportion of debris to water of 2 : 1 and as much as 3 : 1.

In March, 1934, engineers of the Los Angeles County Flood Control District measured five peak flow cross sections of Pickens Canyon, all within a distance of 140 ft, the tributary drainage being 1.74 sq miles. The areas per square mile were as shown in Table 6. These values check closely with the author's cross sections C-3 to C-5 (Table 1(*a*)).

TABLE 6.—CROSS SECTIONAL AREAS, PICKENS CANYON, MARCH, 1934
(Catchment Area, *M*, Equals 1.75 Sq Miles)

Description	Sections					Mean
	a	*b*	*c*	*d*	*e*	
Cross-sectional area, in square feet	400	705	602	727	565	600
Hydraulic radius, *R*, in feet	7.22	10.22	9.85	9.70	8.44	....
Area, in square feet per square mile (direct proportion)	229	400	344	415	323	342

Under conditions prevailing in these small watersheds during the flood, the computation of velocities with formulas by Kutter or Manning would lead to erroneous results. These formulas apply to the flow of water, whereas the runoff of the New Year's flood was a viscuous mass of unassorted debris. The writer knows of no experimental determination of "n" with bulking of 2 : 1 or 3 : 1.

No information is available as to the time and manner of the collapse of the numerous check dams. Suffice it to say that after the flood the canyons were fairly clear of debris and of checks.

Evidence was found of high velocities in all canyons. These have been computed by the author from the location of splashes from the muddy water, at 25 ft per sec; but these splashes may have occurred prior to the removal of checks. There were also indications of a protracted heavily loaded peak flow evidenced by tree trunks 8 in. or more in diameter, which, at points some distance above the stream bed, had been abraded to a depth of several inches.

Estimates of average velocities during the peak of flood were made by the writer, based upon circumstantial evidence. Reports of eyewitnesses indicate that the destructive peak flow arrived in Montrose, Calif., about 15 min after leaving the mouth of canyons. The period of intense rainfall started at 11:45 p.m. on December 31, whereas the peak flow reached Montrose between 12 and 12:15 a.m. on January 1.

Summarizing available data, it was concluded that the average velocity of the peak flow of the bulked material was 10 to 15 ft per sec, and that the proportion of debris to water may have varied from 1 : 1 to $2\frac{1}{2}$: 1, or even higher.

It is believed that the mud stream remained intact as a highly viscuous mass from its inception in the canyon and through the ravine below the mouth and down to the debris cone where it was dispersed by obstructions in its path. After this mass came to rest, the water drained away and found its way to the intercepting channel of Verdugo Creek. It is probable that in the canyons and ravines, during this phase of the flood, a discharge of free water occurred at higher velocities on top of the mud stream and more or less separated therefrom. These would account for waves and walls of water observed by witnesses.

On the basis of estimated mean peak flow velocities of 10 to 15 ft, cross sections measured in Pickens Canyon, with a drainage area of 1.75 to 1.80 sq miles, the discharge given in Table 7 is obtained. These are high discharge

TABLE 7.—COMPARISON OF DISCHARGE QUANTITIES, PICKENS CANYON

Description	AUTHOR'S ESTIMATE		WRITER'S ESTIMATE	
Average velocities, in feet per second	10	15	10	15
Catchment area, M, in square miles	1.78 to 1.80		1.75	
Cross-sectional area, in square feet	641*		600†	
Discharge, in cubic feet per second	6,410	9,615	6,000	9,000

* Mean of Sections C-3, C-4, and C-5, Table 1. † Mean of Sections a, b, c, d, and e, Table 6.

values, even for short transient peaks; they fall on or outside the enveloping curves developed by different writers.

In 1925, C. S. Jarvis, M. Am. Soc. C. E., reported data[26] from which it appears that, for 2 sq miles of watershed, $Q_{max} = 7{,}000$ cu ft per sec per sq mile. Assuming the watershed of Pickens Canyon, tributary to the peak cross section given in Table 7, to average 1.78 sq mile, the Jarvis enveloping maximum would be, for Pickens Canyon, $Q = 7{,}000\sqrt{1.78} = 9{,}350$ cu ft per sec.

In 1939, William P. Creager,[27] M. Am. Soc. C. E., shows an enveloping curve for that year which, for watersheds of 2 sq miles, gives a flood peak of 4,000 cu ft per sec per sq mile. Applying this value to Pickens Canyon, and on the basis of direct proportion to the watershed, the probable peak flow would be less than a value of $1.78 \times 4{,}000 = 7{,}120$ cu ft per sec.

In order to hold estimates of Pickens Canyon peak flow within the aforementioned enveloping curves, the computer must either assume velocities of less than 15 ft per sec, or smaller cross-sectional areas. It is believed that the latter assumption gives results more consistent with the observed nature of the peak flow and its disastrous effects.

Volume of Debris Removed.—Surveys of Pickens Canyon before and after the flood permit approximate computations of removed debris. Mr. Troxell[25] estimated this volume at 72,000 cu yd whereas the writer estimated it at 98,000 cu yd. The latter is at the rate of $\frac{98{,}000}{\sqrt{1.78}} = 73{,}000$ cu yd per sq mile.

The Los Angeles County Flood Control District has adopted the plan of locating debris storage basins at mouths of canyons of the Sierra Madre. These basins are cleared after each flood. The required storage capacity is a function of the following factors: The debris discharge of a major flood; the interval between the floods of a season; and the capacity of the excavating equipment that is available for the clearing of a basin immediately after a flood.

TABLE 8.—ANALYSIS OF THE RECORD CRITICAL FLOOD YEAR

Date	Precipitation*	Flood character	Remarks
October 23, 1889	3.62	Moderate	Washouts on railroads; unprecedented rainfall for this time of year; streets flooded
December 12–15, 1889	4.30	Heavy	Traffic paralyzed by washouts; streets flooded
December 25, 1889	3.82	Heavy	Flood scarcely less severe and disastrous than that of 1884; harbor silted
January 26, 1890	4.17	Heavy	Streets converted to rivers; traffic suspended; harbor silted

* Precipitation in 24 hr, in inches.

The critical flood year of record is believed to be that of 1889–1890, for which Ford A. Carpenter has analyzed flood conditions as shown in Table 8.[28]

[26] "Flood Flow Characteristics," by C. S. Jarvis, *Transactions*, Am. Soc. C. E., Vol. 89 (1926), Plate IX, facing p. 994 (see enveloping curve marked "$Q = 10000\sqrt{M}$; modified Myers maximum 100%").

[27] "Possible and Probable Future Floods," by William P. Creager, *Civil Engineering*, November, 1939, p. 670, Fig. 6.

[28] "Flood Studies at Los Angeles," by Ford A. Carpenter, *Monthly Weather Review*, Washington, June, 1914, 42: 385–391.

The occurrence of three major floods within a period of 45 days makes the feasibility of clearing a series of basins after each flood problematical. It is reasonable to assume that debris discharge would be a maximum during the first flood and less with each succeeding storm. However, it is believed that under the conditions prevailing in the foothills of Southern California a basin capacity of 300,000 cu yd per sq mile should be provided.

Check Dams.—The failure of hundreds, possibly thousands, of check dams during the flood of January, 1934, was responsible for the Resolution passed by a conference of engineers, foresters, agriculturists, and investigators, held under the auspices of the Los Angeles Section of the Society, in 1935, and reported in the Progress Report of the Committee of the Irrigation Division on "Conservation of Water."[29]

In its five conclusions the Conference expressed its disapproval of check dams for the regulation of capital floods and advocated permanent debris dams in mountain watersheds, properly designed and constructed. It is believed that the substance of these conclusions has generally been accepted, at least in Southern California, and that check dams as a major flood control measure are no longer built in the steep mountain watersheds.

HAROLD C. TROXELL,[30] ASSOC. M. AM. SOC. C. E., AND R. STANLEY LORD,[31] JUN. AM. SOC. C. E. (by letter).[31a]—Shortly after the very severe storm of January 1, 1934, the writers were called upon to make an investigation and prepare a flood report[19] for the U. S. Geological Survey. After a thorough inspection of the area it was apparent that the flood flows resulting from this storm were entirely unlike those that ordinarily occur in this area. Unusually large cross sections, similar to those shown in Table 1, were found, and the writers' explanation was given to account for the large sections. On the basis of the evidence found in the area, the explanation made at that time seems more plausible than that indicated by the author.

As the phenomenon experienced during this storm was more or less similar in all the canyons draining the burned-over area tributary to the La Cañada Valley, the investigation was confined mainly to Pickens Canyon, which is considered typical. Numerous check dams had been built in the stream beds of practically all of the streams in this area. A typical view is shown in Fig. 10. In Pickens Canyon, 182 check dams had been constructed in the 17,000 ft of main channel lying between the 1,550-ft and 4,000-ft contours, and additional check dams had been built in the tributaries. The stream-bed slope, in this 17,000 ft of main channel, varies from 7% at the lower end to about 37% at the upper end. The check dams averaged between 5 and 10 ft in height and were instrumental in reducing, to a certain extent, the effective slope of the channel. This tended to produce slower velocities, causing deposition of debris above each of these structures. However, at times of extreme flood the

[29] *Proceedings*, Am. Soc. C. E., December, 1935, p. 1490.

[30] Associate Hydr. Engr., Water Resources Branch, U. S. Geological Survey, Los Angeles, Calif.

[31] Asst. Hydr. Engr., Water Resources Branch, U. S. Geological Survey, Los Angeles, Calif.

[31a] Received by the Secretary March 8, 1940.

[19] "Flood in La Cañada Valley, California, January 1, 1934," by Harold C. Troxell and John Q. Peterson, U. S. Geological Survey *Water Supply Paper No. 796-C*, 1937.

slope tends to return to the normal slope of the stream channel. The deposition of material above each of these structures tends to increase the velocities, due to smoothing of the channel; but this tendency is more than offset by the obstruction caused by the heavily rooted plant life that develops in the deposits.

Pickens Creek flows in a channel carved in bedrock above about the 2,000-ft contour and in a channel that is cut through the debris cone below that point. An inspection of the bedrock sections showed that the check dams, together with the debris deposits above these structures, had been completely washed out of the channel during the New Year's flood. Profiles obtained before and after this flood showed that, for a distance of 6,200 ft upstream from the 2,100-ft contour, the channel had been lowered as much as 15 ft in some sections, with an average lowering of 6.5 ft. Practically all of the debris that had accumulated

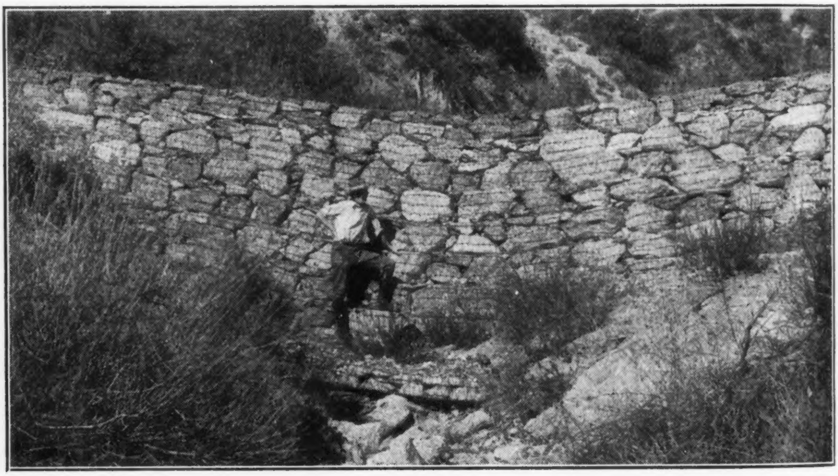

FIG. 10.—TYPICAL CHECK DAM

in the canyon during a period of several years of relatively light runoff was removed from the canyon in this single flood. Based on these two profiles it was estimated that about 72,000 cu yd of stream-bed deposits were removed from the canyon sections of this 1.6-sq-mile drainage area and deposited on the valley below.

Courtlandt Eaton,[32] M. Am. Soc. C. E., has shown a typical set of four adjacent cross sections taken in Pickens Canyon after the New Year's storm. He further states[33] that, although the highest water marks were made when the debris flows were moving very slowly, or not at all, the final lowest channel sections were caused by the later and receding stream flows of higher velocities.

In discussing Mr. Eaton's paper, E. I. Kotok and C. J. Kraebel[34] show a photograph of an additional section in Pickens Canyon. This cross section,

[32] "Flood and Erosion Control Problems and Their Solution," by Courtlandt Eaton, *Transactions*, Am. Soc. C. E., Vol. 101 (1936), p. 1325.

[33] *Loc. cit.*, p. 1324.

[34] *Loc. cit.*, Fig. 15, p. 1352.

together with the sections by Mr. Eaton, compare quite favorably with those given in Table 1. As indicated by Mr. Eaton, it is contended that these large cross sections are largely the result of a debris movement.

As a result of his study on debris movement G. K. Gilbert[35] states that if a stream, loaded to its full capacity, reaches a point at which the slope is less, it becomes overloaded with reference to the gentler slope and part of the load is deposited. If a fully loaded stream reaches a point where the slope is steeper, its enlarged capacity causes it to take more load which, in turn, erodes the bed. If the slope of a stream bed is not adjusted to the discharge and to the load it must carry, "the stream continues to erode or deposit, or both, until an adjustment has been effected and the slope is just adequate for the work."

Thus Nature attempts to maintain a balance between slope, debris load, and discharge. Canyons such as Pickens are very irregular in alinement, cross section, roughness, and slope, with the result that the capacity of a given discharge to carry debris varies considerably from point to point. For example, an exceedingly rough section, a constricting section, or a sharp bend in alinement might cause considerable loss in velocity. With this loss in velocity there is a corresponding loss, but of a much greater magnitude, in the capacity of flow to transport debris; and, as a result, if the stream is loaded to capacity, deposition occurs at or above these controlling points and will continue until the slope is increased or friction factor reduced to such a point that the transporting capacity of the stream is equal to the capacity in the sections upstream. Equilibrium, once established, will continue as long as the debris load and rate of discharge remain constant. With a runoff as unstable as that occurring during the New Year's flood, periods of equilibrium would be of a very short duration.

For many years prior to this storm Pickens Creek had discharged practically clear water on the debris cone. Consequently, there had been a tendency to maintain, upstream from the cone, the flattest possible stream-bed slope that the alinement of the canyon and the roughness of the channel would permit. As stated elsewhere,[36] in a deep, irregular canyon, such as that of Pickens Creek, the normal balancing of debris deposits and channel slopes that might be suitable for the movement of the usual small debris loads develops a channel that is unsuitable and out of balance for the infrequent large flows with their attendant debris movements. With the continuous and heavy rainfall that characterized the New Year's storm, much debris was delivered to the stream channel. In order to transport this abnormally large debris load, the stream had to increase its transporting capacity, either by smoothing its bed or increasing its gradient, or by a combination of the two processes.

The rainfall characteristics of the New Year's storm were such that there were a number of sharp peak runoff periods prior to the midnight storm. Each of these periods of runoff had a tendency to build up and scour out the debris deposits above each of the controlling sections in the channel. The net result of these operations was the gradual movement of the debris load down the

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[35] "Hydraulic-Mining Debris in the Sierra Nevada," by G. K. Gilbert, *Professional Paper No. 105*, U. S. Geological Survey, 1917, p. 26.

[36] "Flood in La Cañada Valley, California, January 1, 1934," by Harold C. Troxell and John Q. Peterson, U. S. Geological Survey *Water Supply Paper No. 796-C*, 1937, p. 77.

canyon to the flatter and broader sections of the debris cone. G. H. Cecil[37] has reported local observers to the effect that the early runoff transported relatively little mud and silt. According to Mr. Cecil, the first reported loss of life occurred in Montrose at about 9 p.m. (December 31, 1933), at which time the flood was said to have been comparatively free of debris.

Thus, after almost 12 hr of continuous rainfall, there was little movement of debris across the debris cone; apparently it was stored in the lower reaches of the canyon section or the upper part of the debris cone. From available information it appears that practically all of the debris was moved across the debris cone and deposited in La Cañada Valley as a result of a very heavy shower or downpour that occurred just before midnight on December 31, 1933.

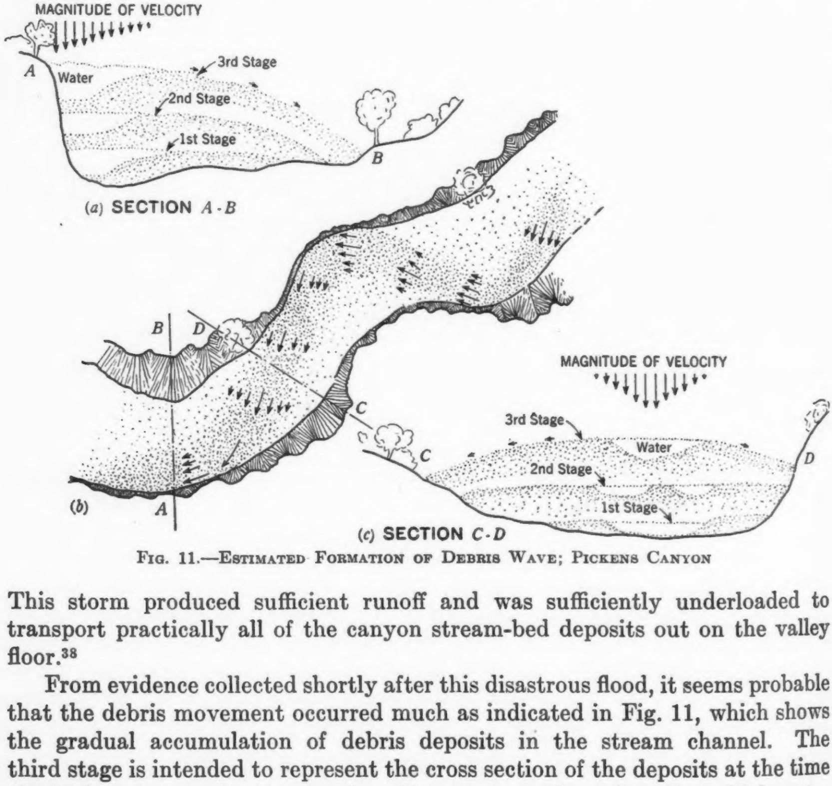

FIG. 11.—ESTIMATED FORMATION OF DEBRIS WAVE; PICKENS CANYON

This storm produced sufficient runoff and was sufficiently underloaded to transport practically all of the canyon stream-bed deposits out on the valley floor.[38]

From evidence collected shortly after this disastrous flood, it seems probable that the debris movement occurred much as indicated in Fig. 11, which shows the gradual accumulation of debris deposits in the stream channel. The third stage is intended to represent the cross section of the deposits at the time the high-water marks were made. It appears evident that these high-water marks may or may not have been made at the time of maximum discharge.

At points in the channel, such as section *A-B*, Fig. 11, the main thread of the flood is trained by the section immediately upstream so that it tends to hug the outside of the bend. As deposition must take place along the boundary between the moving water and debris, and debris at rest, there is a gradual

[37] "Fire and Flood," by G. H. Cecil, Conservation Association, Los Angeles County, 1934.

[38] "Flood in La Cañada Valley, California, January 1, 1934," by Harold C. Troxell and John Q. Peterson, U. S. Geological Survey *Water Supply Paper No. 796-C*, 1937, p. 83.

FIG. 12.—EAST BANK OF PICKENS CREEK AT ORANGE AVENUE; MATERIAL FROM THE DEBRIS WAVE PASSED OVER THE TOP OF THIS 23-FT BANK

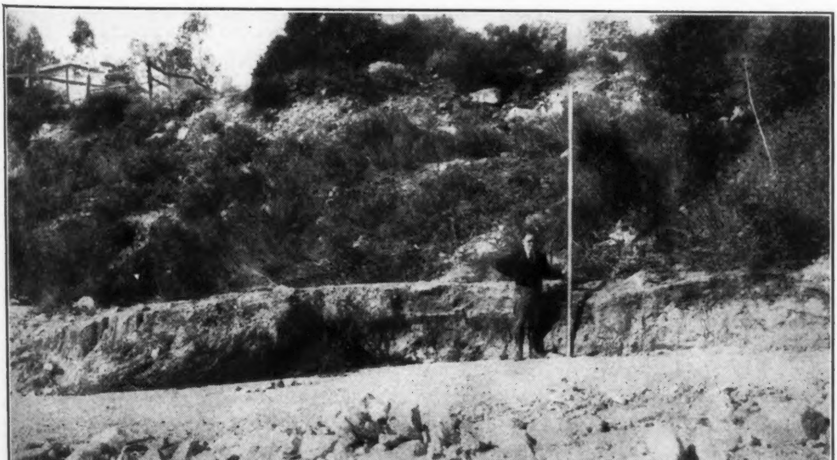

FIG. 13.—WEST BANK OF PICKENS CREEK AT ORANGE AVENUE; THE HIGH-WATER MARK OF THE FLOOD WAS 18 FT LOWER THAN THAT ON THE OPPOSITE BANK

building up of the deposit on the outside of the bend. For example, near the end of Orange Avenue in Pickens Canyon, a section was found very similar to that indicated by section *A–B*, in which the high-water marks indicated a difference of 18 ft between the bank elevations on the two sides of a channel less than 70 ft wide. Figs. 12 and 13 show both the east and west banks of this section. The high-water marks and debris deposits on the top of the east bank were 23 ft above the bed of the stream when the photograph was taken. On the other bank the high-water marks are only about 3 ft above the bed of the stream. It has been suggested by the author that this difference in elevation might be the result of superelevation. However, the composition of the banks of the channel was such that it seems quite unlikely that they could have withstood the eroding action of the stream flow if the velocity had been sufficient to produce a superelevation of this magnitude.

Fig. 14 shows the marking on the outside wall of a bend in the channel. This point is comparable to the point marked *A* in Fig. 11(*a*). Note how the rapidly moving water and debris have scoured the bank smooth; and note also the lack of channel erosion. Thus the stream flow appears to have been loaded to capacity at the time these marks were made; otherwise the channel would have been badly undercut and eroded. Figs. 12 and 13 would seem to indicate that not until the final stages of the debris movement was the runoff sufficiently underloaded to start the erosion process.

The general longitudinal section of the debris movement is shown in Fig. 16. The length of this section is sufficient to represent the time of passage at any given point of the major part of the debris movement. At a section such as that marked *C–D* the stream flow has the maximum transporting capacity. Because of poor flow conditions, such as flatter slopes and the distribution of the flow into a number of channels, a part of this capacity is lost before reaching section *A–B*, with the result that deposition takes place. The downstream movement of the debris is obtained by the scouring effect at the upper section and its movement to points farther downstream. Fig. 15 shows a section such as *C–D* in Fig. 16. It should be noted how the natural process of debris deposition among boulders in the channel favors an increase in capacity to transport debris (see Fig. 16). At the time of the peak discharge this deposit was probably almost level across, with a shallow, fast-moving, heavily debris-laden stream on its surface. Subsequently a smaller, but underloaded, discharge had removed a large part of the deposit.

Sections of debris waves, like the one indicated in Fig. 16, were entrapped at numerous places along the channel, due to irregularities in alinement. Such a section is shown in Fig. 17. The upper half of this figure shows the deposit left by the debris wave. A close inspection will show a marked contrast between the debris-wave deposit and gravel and rock deposit immediately below it. The gravel and rock deposit, composed in the main of recently eroded material, is remarkably free from fine particles. Immediately below the gravel and rock is a layer of charcoal and ash which was probably deposited on the original stream bed during a storm that occurred on December 13 and 14. This is interesting for several reasons. First, it shows that the debris material was composed largely of the finer materials. It was the presence of these finer particles that made the movement indicated in Figs. 11 and 16 possible. Mr.

FIG. 14.—SCOUR OF SIDE WALL IN PICKENS CREEK CHANNEL. NOTE THE LACK OF EROSION

FIG. 15.—STREAM-BED DEPOSITS IN PICKENS CREEK, SHOWING HOW A SMALL UNDERLOADED STREAM FLOW HAD REMOVED MUCH OF THE DEPOSIT

Gilbert[39] found that the addition of the finer material to the stream flow tends to facilitate the movement of the larger particles; and also that the packing together of larger and smaller grains tends to reduce the percentage of voids, which suggests that the percentage of voids, used inversely, might serve as a sort of index of mobility.

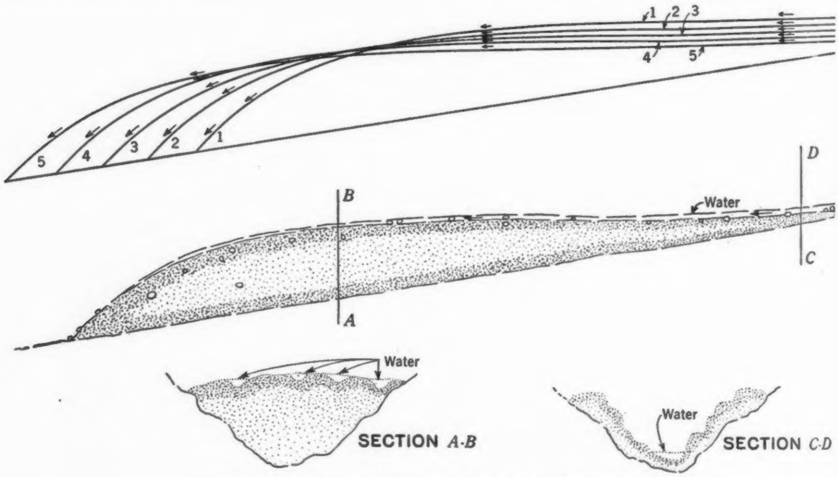

FIG. 16.—TYPICAL METHOD OF DEBRIS MOVEMENT

It is certainly much easier to conceive that a smaller force would be required to develop movement between the particles composing the debris wave, as indicated in Fig. 17, than in deposits immediately below it. It is the writers' opinion that a debris movement, such as indicated in La Cañada Valley on New Year's day, is dependent on the presence of these finer particles.

In Fig. 18 the compositions of the following seven samples are plotted:

Curve	Description
A	Deposit 3 ft deep in a house on Pickens debris cone. Boulders as large as 18 in. were common in this deposit. It showed the poorest degree of sorting and was likely deposited as a mudflow.
B	Fine-grained lens in a rudely lenticular gravel deposit at Pickens wash and Montrose Boulevard.
C	Rudely stratified deposits 4 ft deep with scattered boulders as large as 3 ft, from Halls Canyon overflow.
D	Fine-grained lens from culvert deposit at same location as the sample in curve *B*. This material probably flowed as a viscous mass.
E	From debris deposit (shown in Fig. 17) in Pickens Canyon just above Foothill Boulevard. Altitude, about 1,600 ft.
F	From debris deposit in Pickens Canyon at altitude of about 2,100 ft.
G	Typical bed load of the Santa Ana River.

[39] "The Transportation of Debris by Running Water," by G. K. Gilbert, *Professional Paper No. 86*, U. S. Geological Survey, 1914, p. 179.

Six of these samples were taken of the debris deposits found in La Cañada Valley; the other is that of the bed load of the Santa Ana River near Prado, Calif. The data pertaining to samples *A*, *B*, *C*, and *D* were collected by W. D. Chawner.[40]

It will be noted that the bed-load sample taken on the Santa Ana River (curve *G*) contained only 3.5% of material less than 0.01 in. in diameter. This is to be expected as the finer particles are carried off as a suspended load. However, in the samples collected in La Cañada Valley area, five showed a content of 18% to 22% of particles less than 0.01 in. in diameter. Not all of the finer particles were carried off as a suspended load, probably due to the supply being considerably greater than usually encountered in this area. In fact, this large content of fine particles might indicate that there was little of the segregation usually found in flood flows, and that the finer particles have been deposited as freely as the larger. Of the nine samples collected by Mr. Eaton[41] in the vicinity of Montrose, six contained from 19% to 26% of particles less than 0.01 in. in diameter.

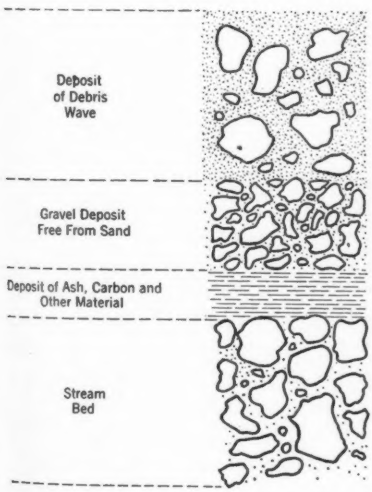

FIG. 17.—COMPOSITION OF DEBRIS DEPOSITS IN CHANNEL OF PICKENS CREEK (ALTITUDE, ABOUT 1,590 FT)

There was considerable more variation in the composition of these samples for particles greater than 0.1 in. in diameter. The analysis of the six samples collected in La Cañada Valley showed the debris deposits to contain from 4% to 54% of particles in excess of 0.1 in. in diameter. The samples by Mr. Eaton show a similar composition except possibly with less extreme variation. Six of Mr. Eaton's nine samples showed material in excess of 1.5 in. in diameter.

[40] "Alluvial Fan Flooding—The Montrose, California, Flood of 1934," by W. D. Chawner, *American Geographical Review*, Vol. XXV, 1935, p. 261.

[41] "Flood and Erosion Control Problems and Their Solution," by Courtlandt Eaton, *Transactions*, Am. Soc. C. E., Vol. 101 (1936), p. 1322.

Mr. Chawner,[42] in discussing his debris samples, states that complete analysis of the debris was impractical inasmuch as boulders of tremendous size were found associated with a mixture of material ranging down to fine sand. He further states that, in general, the coarseness of the material decreased from the apex of the cone and cites the presence of large boulders such as one of 32 tons at 7,000 ft from the mouth of the canyon; another of 23 tons at 7,900 ft; one of 11 tons at 8,400 ft; and one of 5.5 tons at 9,400 ft.

As already indicated, it is difficult to conceive the movement of these boulders, both large and small, out of the canyon except in the presence of debris deposits of a rather fine material. A deposit composed alone of boulders would resist any except a very large shearing force. These same boulders, however, when becoming completely surrounded by large deposits of fine material, offer considerably less resistance to the shearing forces. The presence of the finer particles, together with sufficient moisture, tends to increase the instability of the mass. In general, the analysis of this debris indicates an aggregate showing considerably more of the finer particles than found in ordinary concrete. The usual concrete aggregate under the influence of 15% to 25% of moisture is known to flow readily down slopes as flat as 10%. During the earlier phase of this storm the stream-bed deposits, together with the material eroded from the canyon walls, had become completely saturated. It is possible that under the influence of additional water from a medium-sized shower these deposits might become unstable in part. As indicated in Figs. 11 and 16, most of the movement was confined to a number of rapidly moving streams of water and debris across the top of the deposit. If the mass of the debris were to become unstable, due to increase in moisture content, then a certain amount of motion would be imparted to the debris directly adjacent to these rapidly moving streams. This movement must have decreased quite rapidly as the distance from the rapidly moving stream flow increased.

In discussing the velocities present during the flood, the author states that "a part of a check dam 4 ft by 4 ft by 18 ft long had been thrown entirely out of the channel." Fig. 10 shows a typical check dam such as used in Pickens Canyon. It is rather difficult to conceive of a structure of this nature being thrown out of the channel. In fact, if it had been rolled over many times it would have disintegrated entirely and disappeared. It is the opinion of the writers that the check dam was buoyed up and moved along in this debris wave under the influence of the rapidly moving threads of water and debris, such as indicated in Fig. 11, but did not at any time come in direct contact with these fast moving streams of water. If it had, it would have been destroyed immediately by rolling over or by impact from boulders and debris traveling at velocities such as indicated by the author. The movement of a similar, but somewhat larger check dam, is shown in the report on the "Flood in La Cañada Valley."[43]

It is not intended to minimize the high velocities deduced by the author, but merely to point out that these extremely high velocities were confined to

[42] "Alluvial Fan Flooding—The Montrose, California, Flood of 1934," by W. D. Chawner, *American Geographical Review*, Vol. XXV, 1935, p. 260.

[43] "Flood in La Cañada Valley, California, January 1, 1934," by Harold C. Troxell and John Q. Peterson, U. S. Geological Survey *Water Supply Paper No. 796-C*, 1937, p. 91.

a very small part of the large cross sections indicated in Table 1. Thus, it is the writers' opinion that the author's so-called "surges" are really "debris waves" such as indicated in Fig. 16. The author calls attention to the lack of conformity between the various sections (Table 1) when expressed as a unit of area. It is rather difficult to conceive why any conformity should be expected. These cross sections will vary considerably, even in relatively short

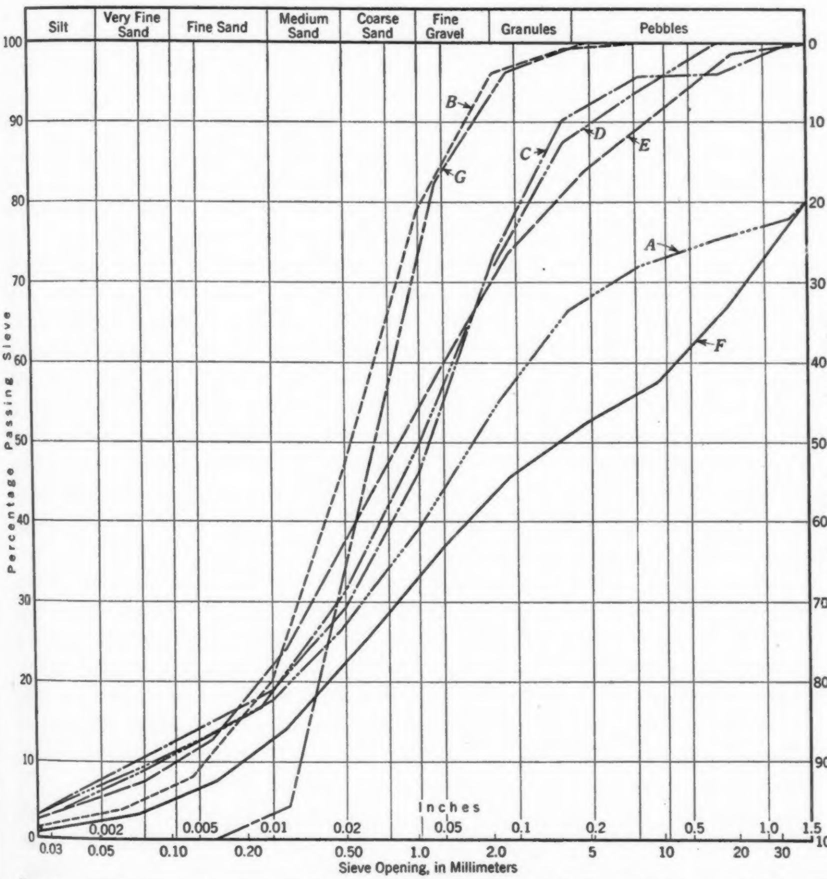

FIG. 18.—MECHANICAL ANALYSIS OF SELECTED SAMPLES FROM LA CAÑADA VALLEY AND SANTA ANA DEBRIS DEPOSITS FROM NEW YEAR'S 1934 STORM

distances and within short time intervals, depending upon the alinement, cross section, and debris movement. In general, the author's flow sections appear to be more of an index of the debris movement during the New Year's storm than an indicator of the stream flow.

Group (*a*) all showed large "flow sections." This is to be expected since all of the areas listed in this group are in the area burned in 1933 and the

debris flow was excessive. Group (*b*) comprises the areas burned in 1927. The "flow sections" for this group are much less than those in group (*a*), mainly because the debris movement was much less. It is natural to expect less debris movement from an area burned in 1927 than from one burned in 1933, because: (1) Much of the fine material had been removed in the 6 years following the burn, and (2) there were 6 years of plant re-growth to hold the fine material back. Group (*c*) shows smaller "flow sections" than either group (*a*) or group (*b*) because the area had not been burned over and, therefore, less erosion resulted. However, because of a large number of slides in this area, the debris movement from it was much heavier than normal. In the case of Hillcrest Canyon (R in Table 1), many of the slides were the result of the numerous roads built in the area. Regardless of the cause, it is known that the debris removed from those areas was much heavier than that from the areas in group (*d*).

If the "flow sections" as used by the author indicate excessive runoff due to "surges," similar conditions of rainfall should produce similar "flow sections" in later storms of comparable magnitude. On March 2, 1938, this area was visited by a storm even more severe than that of New Year's day, 1934. During this 1938 storm, the recording rain gage in Pickens Canyon (elevation 4,250 ft) showed a rate of 2.40 in. per hr for the maximum 5 min, 1.80 in. for the maximum 10 min, and 1.60 in. for the maximum 15 min. Another gage located at an elevation of 2,616 ft in the same canyon showed almost identical records for the 1938 storm. It is doubtful if the rates of rainfall in the New Year's 1934 storm exceeded these values; yet, as indicated by the author, during the 1938 storm none of the so-called "surges" were noted. It is the writers' opinion that the canyon channel, swept clean of all movable debris in the 1934 catastrophe, lacked sufficient of the finer particles to make the formation of a debris wave possible.

Real "surges," where the runoff does tend to develop into a wave action, are more or less a common occurrence in steeper parts of Southern California. However, the prerequisite for the formation of these "surges" appears to be a smooth, fairly steep, and uniform cross-section channel with straight, or nearly straight, alinement. These "surges" are usually confined to artificial channels. One would scarcely consider the irregular, rough, and crooked channel of Pickens Canyon as satisfying these requirements. The same applies to most, if not all, the mountain canyon section in La Cañada area.

In discussing the formation of these "surges" with special reference to artificial channels, Hunter Rouse,[44] Assoc. M. Am. Soc. C. E., states that the reason for the original formation of these disturbances is still a moot question, but indicates that their formation is less likely in irregular channels.

In justifying peak discharges commensurate with the large cross-sectional areas found in Pickens Canyon, the author has implied that under certain conditions it is possible to have rates of runoff in excess of the extreme rates of rainfall. Undoubtedly, there are small drainages in which the conditions governing runoff are such that the rates of runoff may exceed the rates of rain-

[44] "Fluid Mechanics for Hydraulic Engineers," by Hunter Rouse, *Engineering Societies Monographs*, 1938, p. 387.

fall supplying this runoff; but it is believed that this condition is the exception rather than the rule.

In considering the runoff in these steep mountain areas it is believed that the author does not give proper weight to the time it takes the particle of water to travel from the point where it touches ground surface as rainfall until it arrives in one of the major channels of the stream. The runoff from the major part of even these small drainages in the Sierra Madre range reaches the main channels through minute stream channels and the smaller tributaries. One is likely to forget that nearly always these steep stream channels are very rough in cross section and irregular in alinement, so that much of the advantage due to slope is counteracted by loss of energy due to turbulence. From certain work done in connection with a report on the March, 1938, flood, the writers are of the opinion that the velocities of the water in the minute channels have been generally overestimated. This work tended to indicate that the average velocity of the particles of rainfall from the time they hit the ground until they passed the gaging station was from 3 to 4 ft per sec.

Inasmuch as these velocities are very much slower than those present in the main stream channel in each of these areas, it is apparent that travel down the mountain slopes must be considerably slower than these average velocities. Thus it seems likely that the author should have given more consideration to the time it takes the particles of runoff to reach the main channels.

As indicated by this discussion, large cross sections are not always a true index of the peak discharge. From time to time hydrological literature cites very large rates of runoff, ranging (as stated by the author) from 1,000 to 4,170 cu ft per sec per sq mile. If these values are the result of determinations made subsequent to the peak flows where the cross-sectional areas may have been subject to considerable debris movement, they should be used with a degree of caution.

AMERICAN SOCIETY OF CIVIL ENGINEERS

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DISCUSSIONS

EFFECTS OF RIFLING ON FOUR-INCH PIPE TRANSPORTING SOLIDS

Discussion

By David L. Neuman, M. Am. Soc. C. E.

David L. Neuman,[6] M. Am. Soc. C. E. (by letter).[6a]—In his "Conclusions," Mr. Howard states a criterion for determining the advisability of the use of rifling as follows: "Rifling in the discharge line of a dredge will increase the efficiency of the line in cases where the material being dredged through a plain pipe would settle along the bottom in appreciable quantities." Such a condition of insufficient discharge velocity to keep the materials in suspension is generally due to insufficient power on the main pump for the length of discharge line then in use. A booster pump may be placed in the discharge line or, to meet immediate and anticipated needs, the power unit may be replaced by one of higher power. Generally, replacement of the pump is required at the same time.

Repowering a dredge is an expensive and time-consuming process. If the length of discharge line, at the time the insufficient discharge velocities become apparent, is about the maximum length to be used, and if the dredge is to work in silt or clay only a small part of its total working time, an alternate (presented by Mr. Howard) of using a rifled discharge line becomes attractive.

The 20-in. pipe line cutter dredge, *Henry Bacon*, operating under the Wilmington, N. C., Engineer District, was used on maintainance work along the coastal waterways of Georgia and the Carolinas in 1936. Most of the work was in sand, or materials containing a large proportion of sand. With a maximum of 650 indicated horsepower available for the main pump, it was underpowered for the work assigned. To determine whether the use of a rifled discharge line would be of material assistance, tests were made in January, 1937, on the Savannah River, about 10 miles below Savannah, Ga.

The rifled discharge line was equipped with the laboratory type 24 rifling described by Mr. Howard. Each 40-ft length (24 diameters) of 20-in. pipe had

Note.—This paper by G. W. Howard, Jun. Am. Soc. C. E., was published in November, 1939, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: March, 1940, by Fred R. Brown, Jun. Am. Soc. C. E.

[6] Maj., Corps of Engrs., U.S. Army, Grosse Pointe, Mich.

[6a] Received by the Secretary February 5, 1940.

a set of three rifles at each end, spaced at 120° around the section, of 6.83 ft axial length. The rifles were of $\frac{5}{16}$-in. plate, $2\frac{1}{2}$ in. high, having a pitch of 16.67 ft, spot-welded into position. The discharge line, totaling 907 ft in length, was made up of 120 ft of dredge hull piping and 787 ft of exterior floating line. Distributed in the hull piping were four sets of rifling totaling 36 ft in length. A special gage was attached to the hull piping to obtain samples of the material and an indication of the percentage of solids passing through the line. At 15-min intervals discharge velocities were determined by a method (similar to the electro-saline method) which will be referred to as the electro-acid method. Use of concentrated sulfuric acid in lieu of a saline solution was made necessary by the presence of sea water at the site of the work. Steam and fuel consumption, as well as the power output of the main pump engine, was measured for each run. Bottom surveys were made before and after each run to obtain dredge outputs.

The test had to be made during normal maintainance operations and without undue interference with the operation schedule. The intensions had been to make the tests at a place on the inland waterway north of Savannah where only sand would be encountered. Because of a delay in completing preparations for the test, they had to be run at a site on the Savannah River where materials were encountered varying from sand to a stiff combination of sand, silt, and clay, which was difficult to cut and dredge. When operating in the tough, clayey material, the power available on the cutter was too low to feed material to the suction mouth at a rate sufficiently high to load the discharge line fully.

Because of the variation in the materials encountered, the test as a whole was unsatisfactory, in that the relative effectiveness of rifled discharge piping and plain discharge piping was not established. The only justifiable direct comparison, in similar material (sand) and operating at the same pump speed (150 rpm), can be made for one test with plain pipe and one with rifled pipe. The average output with plain pipe was 860 cu yd per hr, place measurement, and with rifled pipe, 1,134 cu yd per hr, place measurement, which denotes an increase of output of rifled pipe over plain pipe of 32%.

No plugging of the discharge line occurred while using either the plain or rifled pipe. In one instance, while operating in clay, the suction line became completely plugged for a distance of about 8 ft immediately in the rear of the suction mouth. It is believed that a suitably designed single rifle placed in the suction line would have prevented the plugging of the suction line. The rifled discharge line was able to pass large balls of clay. No large roots or stumps were encountered. A few weeks after these tests the dredge, with a rifled discharge line, was operated in an area where the material (mud and silt) contained an unusually large number of sawmill slabs and roots. Plugs of the discharge line did occur but, in the opinion of the dredge operators, no more than would have occurred if plain discharge line had been used.

The results obtained in the aforementioned field test of rifled pipe line fully support Mr. Howard in his conclusions and criterion. Had the encountered material been only sand and coarser uncemented materials the value of rifling

to the *Henry Bacon* for obtaining appreciably increased outputs when working in such materials would have been evident.

Acknowledgment.—The data used in this discussion were obtained from the report of the District Engineer at the U. S. Engineer Office in Wilmington. The test was the result of the cooperation of the U. S. Waterways Experiment Station, the Memphis Engineer District, and the Wilmington District. Mr. Howard was present at the test, as representative of the U. S. Waterway Experiments Station.

AMERICAN SOCIETY OF CIVIL ENGINEERS
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DISCUSSIONS

CHANNELIZATION OF MOTOR TRAFFIC

Discussion

By W. W. Crosby, M. Am. Soc. C. E.

W. W. Crosby,[10] M. Am. Soc. C. E. (by letter).[10a]—Highway engineers have received a most valuable disquisition on the channelization of motor traffic, in this paper by Mr. Kelcey. It is true, of course, as the author states, that the subject is not new; but it is of fundamental importance now, and growing more so yearly. Embryonic channelization may be found in the "white line"—reported as established by white stones set in the middle of the pavement of the 400-yr old road between Cuernavaca and Mexico City, Mex. As Mr. Kelcey states, however, more rigid and actual controls of the flexible motor traffic (by channels) and its temperamental drivers are a growing need, increasing with their numbers and the congestion of traffic.

Something must be done to relieve the congestion, to restore the convenience and efficiency of their paved ways, and to minimize accidents to both vehicular and pedestrian traffic.

For many years the writer has been suggesting some of the points so well explained by Mr. Kelcey, and others have likewise discussed them; but Mr. Kelcey's paper seems to collect and arrange the essentials of his topic more concisely, more forcibly, and more simply and intelligibly than the writer has heretofore observed. Highway authorities can digest Mr. Kelcey's paper most profitably, with all the implications its cogent statements suggest to an active imagination.

The writer again feels that there is still a need for emphasis on two or three points. The author writes of the necessity for wider rights of way and for securing them in advance. The writer has presented this argument in the past[11]; but there seem to be yet many authorities who neglect, fatally, to comprehend the principle, even when entirely new routes are being located, and to realize that the desirable widths can often be had just as cheaply as the insufficient ones taken.

Note.—This paper by Guy Kelcey, M. Am. Soc. C. E., was published in December, 1939, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: March, 1940, by Messrs. W. L. Waters, C. J. Tilden, and T. M. Matson.

[10] Cons. Engr., Coronado, Calif.

[10a] Received by the Secretary February 26, 1940.

[11] "Elbow Room," by W. W. Crosby, *Roads and Streets*, October, 1934, p. 362.

Parenthetically, it may be remarked here that not only is it the ultimate development of proper channelization, which is obstructed by insufficient right-of-way takings, but that also the character of the highway and of the development of the adjacent property is degraded or retarded by the establishment of too narrow margins between the high-speed channels ("express-ways" or "freeways") and the private property alongside. Objectionable roadside development, and even the growing aversion to highway noises that drive people from their former homes along a through highway, as well as the depreciation of land values, can be reduced or controlled by proper right-of-way widths and channelization.[12, 13, 14]

Another point the writer would again emphasize is that covered by Mr. Kelcey under the heading, "Human Factors." Motor drivers acquire habits. Their subconscious reactions differentiate them. It is in the public interest to induce, and to cultivate, the habits of good reactions appropriate to the situation. Obscure channelization interferes with (it does not actually destroy) this development. Signs and local ("house" or "ground") rules are only partly effective, or only acceptable in certain cases, especially where the time factor is involved, as is usual in motoring.

Automatic, unavoidable control is infinitely better than signs, rules, or laws toward securing efficient and safe driving.[15] The engineer preaches—for those ends—education, engineering, and enforcement, and there is no question that the last is properly placed. The first two are interrelated, and the priority may be mooted.

To illustrate: The motorist is educated to a subconscious reaction that one turning to the left shall keep to the left in accordance with the usual normal tendencies until that turn is available. Some years ago local regulations in a few places (Washington, D. C., for example) required left-turners to pull off to the right on their green light, stop across the traffic on the cross street, and then swing to the left in the traffic lane with the green light on the cross street. The writer is advised that this practice has been generally abandoned. It seemed abnormal and to require a regrettable amount of mental concentration for those unaccustomed to it.

If "instinct factors" are to prevail, it seems that a left turn demands a left course or inclination preceding that turn. If that is a general rule, then local reversal of it is "out of order"; and yet, on so modern a structure as the "Randalls Island Traffic Circle"[16] traffic turning to the left must first swing to the right. Too many of these special cases will prevent the inculcation of uniform habits. Briefly, the writer would repeat what he has stated elsewhere:[17, 12]

"What looks right may be wrong, but what looks wrong cannot be right."

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[12] Discussion by W. W. Crosby on "The Modern Express Highway," by Charles M. Noble, M. Am. Soc. C. E., *Transactions*, Am. Soc. C. E., Vol. 102 (1937), p. 1117.

[13] "The Express Road and the Highway System," by John S. Crandell, M. Am. Soc. C. E., *Civil Engineering*, October, 1937, p. 690; also, *loc. cit.*, December, 1937, p. 859.

[14] "The Parting of the Ways," by W. W. Crosby, *Roads and Streets*, November, 1936, p. 27; also *Civil Engineering*, April, 1938, p. 276.

[15] "Economic and Engineering Problems of Highway Location," by W. W. Crosby, *Transactions*, Am. Soc. C. E., Vol. 91 (1927), pp. 1012 and 1014; also "Safety and the Highway Machine," by W. W. Crosby, *Roads and Streets*, March, 1936, p. 48.

[16] *Civil Engineering*, January, 1940, p. 51.

[17] "Notes on Highway Location," by W. W. Crosby, Gillette Publishing Co., Chicago, 1928, p. 78.

In this connection also may be taken up the question of left turns in intersections as discussed by Mr. Kelcey under that heading and illustrated by Figs. 12 and 13. If the driver adheres to the natural inclination to the left just before the turn (so as to avoid crossing or interfering with following traffic), it may be desirable to "go around the button" for more than one reason. It is in the interest of the extension of present habits to do so. It avoids meeting head-on traffic in the latter's lane on the cross street. It places the left-turner in the proper lane on the cross street (Fig. 40). If the turn "inside the button" is permitted, the tendency is for it to degenerate into the road corner shown in Fig. 14. To allow different practices at different places interrupts the cultivation of the "orderly" procedure desired.

In establishing a rule for the ordinary traffic the highway engineer must exclude from the general consideration the long turning-radius needed by an exceptional vehicle. Often, this vehicle can turn just as well around the button by keeping or swinging to the right before turning left as is done with street-car tracks in some cases of narrow streets.

Channelization would seem to offer a solution that would care for the conflicting interests acceptably in most cases, and some forms are proposed in Figs. 19 and 21. Until it is more widely established, it might be well to insure greater uniformity regarding the left turn, and the writer can only say that he does not yet favor the turns of Fig. 13 except under the direction of a traffic officer.[18]

In Fig. 19 should there not really be two more islands at *B* and at *D*?

Figs. 31, 32, 33, 35, 36, and 37 do not seem to provide adequately for left turns, although they could quite readily be made to do so by channels through or around the islands, which otherwise seem effective. Possibly the author meant his assumption of "no left turns" for Fig. 28 to apply to these also.

For years the writer has been convinced that a widespread, intelligent use of the "rotary system" for relieving objectionable traffic conditions arising from the conflict of traffic streams should be tried. Therefore, he welcomed Mr. Malcher's[6] masterful exposition and development of the principle, which was shown to be of far wider application than to street intersections alone. He now applauds, respectfully, the suggestions and statements of Mr. Kelcey along this line, particularly those under "Divided Highways," "Grade Separations," and "Conclusions."

(One sometimes wonders how much of the present pressure for expensive grade-separation construction comes from producers of the materials and machinery used therefor, and how many times large expense might be avoided by a rotary solution of the problems.)[19]

Channelization might be said to be the "twin"—or, at least, a "baby brother"— of transportation, but its development has not kept pace with the latter. More attention could profitably be paid to channelization so that the two could most effectively perform "in double harness" for the general benefit.

[18] Report of Committee on Highway Traffic Analysis, *Proceedings*, 9th Annual Meeting of the Highway Research Board, December 12–13, 1929, p. 97.

[6] "The Steady-Flow Traffic System," by Fritz Malcher, Harvard City Planning Studies, Vol. IX, 1935.

[19] Discussion by W. W. Crosby on "The Modern Express Highway," by Charles M. Noble, *Transactions*, Am. Soc. C. E., Vol. 102 (1937), p. 1118.

Convincingly, Mr. Kelcey emphasizes the capabilities of channelization. The writer would submit a caution, in view of the tendency to rush to extremes with any remedy—sulfanilimide or war to save the world for the democrats.

Channelization is not a "cure-all." It has its objectionable features, as may be illustrated by the "freeway," which affords protection along its sides to the traffic stream, but at the same time interferes with the use and development of adjacent land.[20] Where width of right of way is unobtainable, the narrowly restricted channels of the freeway may be justified as is an elevated roadway or subway in certain cases. When width can be had (or is reserved by forethought and previous action) the propriety of widening the channels, of increasing their number, or of otherwise providing greater freedom for traffic and development or for other purposes as by a "Pairway," must be recognized.[20, 21]

The broader aspects of channelization have been most interestingly illustrated by John F. Curtin,[22] Jun. Am. Soc. C. E.

Efficient and safe transportation necessitates suitable routing, alinement and grades, and channelization including lane arrangements and provision of closed channels or "pipes" for the vehicles and the pedestrians.

If a road is regarded as a "bridge" between two "bridge-heads" or "terminals," then "channels" of varying degrees of tightness will be useful in collecting and in distributing the bridge traffic.

The writer still believes that the motor (and the driver) should be allowed the greatest freedom of action, on the driver's responsibility, up to the point where another individual may be affected. From then on the needed control should be as automatic as engineering can devise it, as intelligent and habitual as education can cultivate it, and as efficient as enforcement can make it. Channelization will be an important factor in the program.[23]

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[20] "Freeways and Pairways," by W. W. Crosby, *Planners Journal*, May-June, 1937, p. 62.

[21] *Civil Engineering*, July, 1937, p. 518.

[22] *Proceedings*, Am. Soc. C. E., November, 1939, p. 1527.

[23] Report of the *Proceedings* of the Hague Cong. (1938) of the Association Internationale Permanente des Congrès de la Route, p. 186 *et seq.*, and p. 275 *et seq.*

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DISCUSSIONS

WATER SUPPLY ON UPPER SALT RIVER, ARIZONA

Discussion

BY LEROY K. SHERMAN, M. AM. SOC. C. E.

LEROY K. SHERMAN,[12] M. AM. SOC. C. E. (by letter).[12a]—The record of observed stream flow in the Upper Salt River of Arizona is presented in this paper. In making estimates of future flows or storage requirements for hydroelectric purposes, it has been customary to predicate the future upon the past. The use of duration and frequency curves indicates how big or how little the flow may be and how often, on an average, such events may be expected. The author has had the courage to depart from the orthodox procedure and go into the future for at least 10 years. He makes specific prophecy when the flow events will occur. He compiles the mass diagram based in part on his forecasted 10-year hydrograph. Admittedly, a definite knowledge of flow conditions for the first 10 years of the life of a water-power plant would be valuable. The questions arise: Is the author's procedure safe for the requirements involved? What is the difference in amounts between the orthodox and the author's estimates? Does this affect the design?

Under the heading "The Probable Future Hydrograph" the author lists five control conditions. Conditions (*a*) and (*b*) are in accord with usual practice. Condition (*c*), "The minimum 2-year and 3-year drought sequence must exceed 400,000 and 700,000 acre-ft respectively," does not appear logical to the writer, despite the author's statement under the heading "Types of Years." Why should not these values be as low as, or lower than, the flow record for the 3 years 1902, 1903, and 1904—namely, less than 200,000 acre-ft? Condition (*d*), "The general trend * * * must follow the curve of sunspot activity," is a major feature of this paper.

It has been facetiously said that the sunspot theory shows remarkable correlation with precipitation after both events have happened. The writer has noted statements of one effect of sunspots in a part of the country and simultaneously the opposite effect in another part of the country. He is unable

NOTE.—This paper by John Girand, Assoc. M. Am. Soc. C. E., was published in December, 1939, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: March, 1940, by Dana M. Wood, M. Am. Soc. C. E.

[12] Cons. Engr., Chicago, Ill.

[12a] Received by the Secretary February 27, 1940.

to reconcile this influence of the sun upon the earth, which is a little sphere rotating 93,000,000 miles away, at the rate of 1,000 miles per hr.

Nevertheless, the engineer should seriously consider any facts that seem to furnish some vision as to the definite or probable time of occurrence of meteorological events. He is expected to be a prophet. He will be a better prophet when he can say "the event will come within five years" instead of "the event may come some time during the next two hundred years—maybe next month."

In 1935, Sydney M. Wood correlated sunspots with corresponding elevations of the surface of Lake Michigan.[13] On this basis he forecast the elevations of the lake until 1949. A comparison with Mr. Wood's forecast and the actual elevations is as follows:

Date	Forecast in 1935	Recorded elevations, U. S. Lake Survey
January, 1936	578.2	577.91
July, 1936	579.4	578.65
January, 1937	578.7	577.78
July, 1937	580.1	578.66
January, 1938	579.0	577.74
July, 1938	579.7	579.55
January, 1939	578.8	578.74
July, 1939	579.8	579.96
November, 1939	579.3	579.35

Forecasts, as good as this, would permit the successful operation of regulating works on the Great Lakes.

With a few examples, like the foregoing, engineers will begin to have some confidence in sunspot forecasting. In the meantime, the theory should be applied with caution and the results compared with the more common procedures. If the forecasting should be in error, it is important to know how much the effect may be on either design or operation. In this connection, it would be an interesting comparison if the author had given the results derived from a mass diagram based only upon the observed hydrograph.

At some time, in the course of events, there will be a 3-year average flow of less than 200,000 acre-ft per year. It has happened in the past. There is no gainsaying that it may not be even less than this. Likewise, the spillway will probably be called upon to carry a greater flood than any contained in the local record of gagings. The Division of Rivers and Floods of the U. S. Weather Bureau has been successful in deriving, for spillway design, a fair value for the maximum storm to be expected upon a basin. May not the sunspot theory be applied for a derivation of the maximum drought period?

It is fair to point out that Mr. Girand has proceeded with caution in delving into the future. In his forecasted 20-year period, his low flow for a year is 129,000 acre-ft versus 119,700 of record, and, for 3 years, 245,000 acre-ft versus 190,000 of record. The relatively small differences seem to be supported by the sunspot forecast. The author has performed a constructive service to the profession in presenting this specific example of forecasting stream flow on the Salt River.

[13] *Bulletin*, Associated State Eng. Societies, Chicago, Ill., October, 1936, pp. 83–102; see Fig. 1, p. 87.

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DISCUSSIONS

ANALYSIS OF LEGAL CONCEPTS OF SUBFLOW AND PERCOLATING WATERS

Discussion

BY MESSRS. SAMUEL C. WIEL, HYDE FORBES, AND RONALD B. HARRIS

SAMUEL C. WIEL,[26] ESQ. (by letter).[26a]—Energetically, the authors offer the accumulating data of a new science. A hundred years ago, when law on waters was beginning, a law writer, then justifiably, wrote:[27]

> "The origin of springs and rivers is one of the mysteries of nature, which human reason has attempted in vain to develope. The inquiry how they are produced, and whence they derive their unceasing supply of water, has been variously considered, and divided the opinions of philosophers more than any other topic in natural history. The opponents in the controversy have enlisted under two leaders, De la Hire who contends that rivers are supplied from the sea, percolated through the pores of the earth, and Doctor Halley who has endeavoured to demonstrate that the clouds alone are sufficient for the supply. Each has had recourse to mathematical calculations, and the results of each have been equally unsatisfactory."

Today, the passages from legal discussions, cited by the authors, show legal cognizance of such developments of hydrology as catchment basins, underground reservoirs, water levels, underground streams, permeability, underflow, and saturation.

This legal hospitality to the data of hydrology indicates more to come as hydrology is patiently presented. The use of ground-water contours, ground-water mounds, and ground-water troughs which the authors expound may be expected to come into legal discussions. These graphic methods do not produce the conditions which they describe, any more than security-market graphs make security prices; but they are evidently an advanced medium of description

NOTE.—This paper by C. F. Tolman and Amy C. Stipp was published in December, 1939, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: February, 1940, by Donald M. Baker, M. Am. Soc. C. E.

[26] Attorney at Law, San Francisco, Calif.

[26a] Received by the Secretary February 13, 1940.

[27] "Watercourses," by Joseph K. Angell, 1st Ed. (1824).

which legal authorities can, and doubtless will, learn from hydrologists. It is fair to assume that some patience will be required, however.

"Hydrology," A. F. Meyer wrote in 1928,[28] "is essentially a new science" and "our present knowledge of the subject is indeed fragmentary and incomplete." The first general treatise published in English devoted primarily to the part of hydrology dealing with ground water, Professor Tolman has noted, appeared in 1937.[29] Naturally, in this newness there are conflicts among hydrologists. Mr. Meinzer[30] writes of localities where "some of the water may be virtually stationary in synclinal troughs or encased lenses"; the authors state (see heading "Percolating Waters: Subsurface Reservoir") that "ground water never occurs as a stationary water body." Baker and Conkling[31] declare that the proper conception of ground water in alluvium is that of "water in a reservoir"; the authors maintain that such comparison is not justifiable. Elsewhere, Professor Tolman has stated that "No water table can exist in impervious material";[32] Meinzer intimates that in most places no material is absolutely impervious, unless, perhaps, he means, at considerable depth. ("Even the compact igneous and metamorphic rocks and the dense clay and shale formations have in most places become sufficiently permeable by weathering near the surface to have a water table."[33]) Naturally, a new science's debates slow its reception.

Law also has its factors for patience. Not all rests with courts. The legislatures share in setting up legal rules. Citing the law's differentiation of "subterranean streams flowing through known and definite channels" from other underground water, the authors (see "Subsurface Water Course") conclude that "courts refuse to accept scientific evidence indicating natural conditions." Although it originated in courts, the differentiation is now frequently prescribed by legislation.[34]

Perhaps the most important moderator on the rate of reception of hydrology into law is the fact that what takes place in courts about water never occurs in the abstract as in the classroom or office, but is part of a drama of opposing sides. Unless the engineer will include human nature in his conception of it, his combination-rate for hydrology and law is certain to be too high.

In the classroom or office one is unopposed. In court, nothing can be determined against persons who have not been brought in as opponents, which leads in practice to a marked and often decisive curb compared to the expansive latitude in the classroom or office. Then, among those brought in, nothing can be determined outside the issues made by preliminary statements and counter-statements of what is intended to be presented. These may be formed from such considerations as length of time the controversy has lasted, repre-

[28] "Hydrology," by Adolph F. Meyer, p. 1.

[29] "Ground Water," by C. F. Tolman, McGraw-Hill Book Co., Inc., 1937, page vii.

[30] "Groundwater in the United States," by O. E. Meinzer, *U. S. Geological Survey Water Supply Paper, No. 836-D*, 1939, p. 182.

[31] "Water Supply and Utilization," by Donald M. Baker and Harold Conkling, 1930, p. 346.

[32] "Ground Water," by C. F. Tolman, McGraw-Hill Book Co., Inc., 1937, p. 225.

[33] "Groundwater in the United States," by O. E. Meinzer, *U. S. Geological Survey Water Supply Paper, No. 836-D*, 1939, p. 174.

[34] California Water Commission Act, Sec. 42; Arizona legislation reviewed in Maricopa County Municipal Water Conservation District vs. Southwest Cotton Co., 4 Pac. (2) 369, 375–377 (1931), 7 Pac. (2) 254 (1932); Oklahoma legislation reviewed in Canada vs. City of Shawnee, 64 Pac. (2) 694, 698 (1936).

sentations that one party has made to the other, the presence or absence of writings, and so on in endless variety that may be more vital to the parties than physical data.

The trial of the issues, after they are formed, is full of adventure. As much as one is unopposed in the classroom or office, in court little that is asserted will be accepted that has not been proved; and for hydrology this means proof by witnesses. Obviously, what a judge may assume to be true, by judicial notice without witnesses, must be kept very limited. "If there is any doubt whatever, either as to the fact itself or as to its being a matter of common knowledge, evidence should be required."[35]

An illustration occurs when one bases a claim upon existence of a known condition of a particular kind, such as a known and defined underground stream, already noted, which the authors discuss. When a litigant contends for a superiority on that score there is no escaping that the facts are in no two places the same. "The point at which an underground stream merges into a body of percolating water is sometimes as difficult to determine as the point at which a river entering a lake ceases to be a river and becomes a lake, and depends upon the facts of each case."[36] It is evidently like the miners' contentions of knowing that their veins of ore run underground into a property where they are being extracted by another. When denying a claim based upon existence of a known and defined underground stream, the ground water is called "diffused" only in the sense of "undefined" by the evidence. The authors seem to be a little too inclined to assume that this bars parties from proving a motion where they can. The writer believes that what it requires is only that the motion and other hydrological characteristics and environment be proved by evidence in each case according to the locality and instance. Property may not be awarded on "no more than a guess or a wish."[37] Would it be agreeable to hydrologists to have a judge conjecture on a locality's hydrology by judicial notice without any witness testifying?

Failure to produce evidence thus reflects less on the court than on the litigants. However, it is not necessarily a reflection because comparatively few water suits are between properties of a value to justify much expense for evidence. When expert evidence is used, moreover, the expert having an effective personality is likely to be preferred, the same as in selecting between possible witnesses to be offered on any other matter. He may then carry something that hydrologists who later read the decision will dislike.

That is not all, however. The other side will have its contrary expert. In the classroom or office, hydrology can be stated abstractly and with indifference; but called to take up a quarrel for one side the lecturer or engineer, like the attorney, begins to warm up a sympathy for the side calling him; his reasoning powers and learning will in turn warm up his sympathy; he will deem it a merit to detect anything new in his side's favor; then he believes honestly that it is the only right side and is as convinced of this as is that side's attorney. When

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[35] Chowchilla Farms vs. Martin (1933), 219 Cal. 1, 25 Pac. (2) 435, 439.

[36] "Water Supply and Utilization," by Donald M. Baker and Harold Conkling, 1930, p. 321. Accord, San Bernardino vs. Riverside, 186 Cal. 7, 198 Pac. 784 (1921).

[37] Evans vs. City of Seattle (1935), 182 Wash. 450, 47 Pac. (2) 984, 986.

the opponent's expert appears with a contradiction, the first expert's zeal for his client passes into defense of himself.

It has been said:[38]

> "These witnesses ought, perhaps, to be selected by the Court, and should be impartial as well as learned and skillful. A contrary practice, however, is now probably too well established to allow the more salutary rule to be enforced, but it must be painfully evident to every practitioner that these witnesses are generally but adroit advocates of the theory upon which the party calling them relies, rather than impartial experts, upon whose superior judgment and learning the jury can safely rely. Even men of the highest character and integrity are apt to be prejudiced in favor of the party by whom they are employed. And, as a matter of course, no expert is called until the party calling him is assured that his opinion will be favorable."

Being said, as this was, in 1870, it shows an early instance of the contention that expert witnesses should be called by the court instead of by the parties. This contention has provoked some discussion on legal lines, but perhaps more on the line that might be called "socialized hydrology," in analogy to the now-current discussion over "socialized medicine." Some approach thereto may perhaps be seen in the authority sometimes given to the courts to refer water cases to the State Water Administration for report. Such authority exists in a number of states.

In all this, the parties, advocates, witnesses, and at times jurors, are men of all degrees of mental and social equipment, temperament, and predispositions. Men are influenced more by their interest, anger, hope, fear, and the like than by rules of law. So is the judge; but, to the degree that he is a good judge, he will be silent and remain skeptical of all the witnesses together until the decision is made, under his oath of impartiality. On appeal, this scene is preserved in the printed record of the trial, with additional plotting and counterplotting to move the judges in the appellate arguments. It is said in a not uncommon instance:[39]

> "From reading the expert testimony presented by appellant, one would be led to believe that the Kaweah delta is rapidly becoming either a swamp or an alkali waste, while the experts for respondents would have us believe that the delta is a veritable Garden of Eden."

The courts necessarily sometimes must take refuge simply in deciding against the party who has the burden of proof.

Hydrology in litigation must survive this very adventurous human journey the same as other litigated matter, before its entry into decision is possible. At least some criticism of the courts must be discounted as the same disappointment of a losing side that is inseparable from a human drama wherein chance of loss is inherent, whatever the subject.

If hydrology's entry into judicial decision is therefore slow or sometimes imperfect, it is fair to assume that its acceleration waits on practical considera-

[38] Grigsby vs. Clear Lake Water Co. (1870), 40 Cal. 396, 405.

[39] Tulare Irrigation District vs. Lindsay-Strathmore Irrigation District (1935), 3 Cal. (2) 489, 45 Pac. (2) 972, 1009.

tions. Among these may be suggested acquainting the smaller litigants with the benefit that hydrologists, as witnesses, can be to them. Such witnesses might also be made available to them in some way at affordable expense. As it becomes systematized by such work as that of Professor Tolman, hydrology will also progress, as a matter of course, toward precision that will make faulty experting more easily refuted. But such considerations will evidently need a little time.

Although the authors emphasize what hydrology has compiled, and the speed with which they would like to see it appear in legal depositions, the writer wishes to emphasize rather the converse service that hydrology can perform. By affirming the great scope of what must continue to be indeterminate, hydrology can clarify the legal consequences from some popular misassumptions.

Much of the unknown, that prevailed when the law of the subject began, will not be eradicated for some time to come, even with the best patience.

Mr. Meinzer—credited by the authors with leadership of the subject—is insistent upon this view. He says the facts or truths involved in ground-water hydrology "are known in only small fragments" and he "has become impressed with the large and difficult field that the subject covers and the variety and complexity of the concepts that it involves."[40] As recently as 1939 he has spoken of "almost sensational revelations, showing that both water tables and artesian pressure are constantly fluctuating in complicated fashion in response to a variety of impinging forces."[41]

Other commentators likewise[42] are usually ready to affirm this residual indeterminateness. Geologists have testified in court that a certain ground-water occurrence was as much a surprise to them as to others.[43] The courts sometimes find that the evidence presented to them discloses that it is difficult even for the most experienced geologists to determine, definitely, ground-water's source, course, or destination under certain circumstances;[44] that how long it will take for a particular ground-water change to come about "we doubt if anybody knows,"[45] or "probably no one knows."[46]

The unknown affords opportunity for unrealities to get belief. Disregard of the residual indeterminateness of the subject readily leads to embracing some form of legal separation of rights by the process of dividing-up water resources into independent parts like land lots.

One form thinks of dividing the water-body or "corpus" into detached masses by vertically projected land-boundary lines.

Will this stand hydrological analysis? How water resources of whatever kind, retaining so much that is indeterminate, can yield such a separation into

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[40] "Outline of Ground-Water Hydrology," by O. E. Meinzer, *U. S. Geological Survey Water Supply Paper, No. 494*, 1923, pp. 1, 2.

[41] "Groundwater in the United States," by O. E. Meinzer, *U. S. Geological Survey Water Supply Paper, No. 836-D*, 1939, p. 212.

[42] Report of Special Advisory Committee submitted to the Secretary of the Interior, May 26, 1936, by National Resources Committee, p. 9.

[43] O'Leary vs. Herbert (1936), 5 Cal. (2) 416, 55 Pac. (2) 834, and cf. Joldersma vs. Muskegon etc. Co. (1938), 286 Mich. 520, 282 Northwestern 229.

[44] Silver King etc. Co. vs. Sutton (1934), 85 Utah 297, 39 Pac. (2) 682, 690.

[45] Berry vs. Shell etc. Co. (1934), 140 Kan. 94, 33 Pac. (2) 953.

[46] Comar Oil Co. vs. Blagden (1934), 169 Okla. 78, 35 Pac. (2) 954, 955.

parts, is difficult to understand. Water resources are compounded of runoff, seepage, evaporation, and plant transpiration, controlled by the wind, rain, heat of the sun, and forces in the depths of the earth, all of which are indivisibles. The writer interprets the paper to convey, emphatically, that all parts of a given water body are inextricably inter-related.

Particularly, they are in motion—the authors insist, always in motion (see heading, "Percolating Waters: Sub-Surface Reservoir")—and even a surface stream has ground water entering or leaving the bed as an important feature. In this connection, Clarence Johnston, M. Am. Soc. C. E., has stated:[47]

> "Streams and lakes are not like land, in so far as private possession is concerned. Land can be measured and privately controlled. Water is constantly shifting, and the supply changes every day."

Motion gives streams the unique character of being a perpetual renewal of transient contents. Unification of a stream's coming and going—and it is more or less the same of all other water resources—exists only subjectively in the observer's eye. Just so the apparent unity which the succession of impressions on the film of a motion picture has on the screen is only a subjective unity. The apparent single objectiveness is the same visual illusion in both instances; and if this is realized there is nothing that answers to a determinate block of water for land-boundary lines to divide into the lots supposed.

It brings to mind a case in which:[48]

> "The discovery of a statue of Pompey, ten feet in length, was the occasion of a lawsuit. It had been found under a partition wall; the equitable judge had pronounced that the head should be separated from the body to satisfy the claims of the contiguous owners; and the sentence would have been executed if the intercession of a Cardinal and the liberality of a Pope had not rescued the Roman hero from the hands of his barbarous countrymen!"

Popular discussion, and law books too, show some unsuspecting efforts to adjust legal rights by dividing the water "corpus" by the geometrical process mentioned. It ought not to be too much to expect of hydrology that it stand against that fanciedly simple legal solution, often entertained by the public for allotting water resources, and by some legal and engineering circles.

One should not be deemed to be going too far in suggesting also that hydrology should stand against the comparable popular solution that professes to divide water resources into legal parts in another way. This assumes to segregate parts from the remainder mathematically according to priority in time of breaking off the part—that is, "prior appropriation." In searching the expressions of the past one finds that natural water supplies have been frequently treated in law as though they were bank accounts as a "corpus" of "appropriable water" to be drawn from in certain amounts by a simple process of subtraction.

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[47] *Transactions*, Am. Soc. C. E., Vol. LXXVI, December, 1913, p. 683.

[48] "Roman Empire," by Edward Gibbon, Ch. LXXI, Everyman Ed., Vol. VI, p. 567.

The multifarious qualifications that have come to surround that extended effort show how this supposed mathematical simplicity of dividing-up the visual illusion is beset with the same hydrologic indeterminates; for example:[49]

> "Administration of underground water involves gathering and studying data. The work is in the office, and decision is not so easy. The cost of obtaining sufficient information to provide a basis for intelligent decision is considerable, and this cost would not be justified where economic conditions do not permit * * *."

In some instances, it has had to involve measuring absorptive capacity of all stream beds in a large county.[50] Elsewhere, constant public records of thousands of well readings are being introduced. Indeed, courts that are careful sometimes find water conditions, whether underground or on the surface, so indeterminate that experiment is compelled. They sometimes find it necessary to reserve jurisdiction so that they may modify their orders, or they find it necessary to appoint commissioners to keep in touch with the obscure or changing situations.

Whoever has charge of legal adjustment on the conception of mathematical division of water resources by priority of "appropriation" of parts, finds himself, sooner or later, in difficulties. It has been one of litigation's most fruitful sources. He comes, through trial and error, to suspecting whether water-division formulas, be they geometrical or mathematical, apparently so simple, are searching in a dark room for a black hat that is not there. He confesses the need for a considerable degree of discretion to bring, out of indeterminate physical circumstances, the best human result that he can in his own way. He needs a considerable degree of latitude to leave formularizing, and apply such disposition as he believes to be "reasonable use" on the part of the one water claimant toward the other; that legal position to which the discussed paper refers by saying (and the writer believes, correctly) that although several states of the West have extended rights by prior "appropriation" to ground water, the general trend "seems to be toward application of equal rights" (see "Part II.—Analysis of a Few Rules of Law").

Since appropriation signifies mathematical division into parts by priorities, the trend from it to equality of standing based upon discretionary "reasonable use" of water claimants toward each other is evidence that hope of dividing-up water resources, whether geometrically like land lots or mathematically by priorities in time, however easy of imagining and of verbally expressing and enacting, is lacking in something under the water authority's experience.[47]

The law is increasingly coming to rely upon how a situation which the water authority faces impresses his good sense. If this is unfortunate—and perhaps it is—the writer ventures to think that it is nonetheless unavoidable, with loyalty to reality.[51]

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[49] "Administrative Control of Underground Water: Physical and Legal Aspects," by Harold Conkling, *Transactions*, Am. Soc. C. E., Vol. 102 (1937), pp. 753, 778–779; also "San Gabriel Investigation," by Harold Conkling, *Bulletin No.* 7, Division of Water Rights, Calif. Dept. of Public Works, p. 14 *et seq. passim.*

[50] "Ground Water," by C. F. Tolman, 1937, p. 179.

[51] "Fifty Years of Water Law," by Samuel C. Wiel (1936), 50 *Harvard Law Review* 252, 16 *Oregon Law Review* 203.

The discretionary authority spoken of (which is to say what is "reasonable") has been, at common law, the courts; but current practice is preferring to put it by legislation into administrative officers in the first instance. In the West, particularly, there are extensive State Water Administrations which, while initiated to achieve mathematical divisions by priorities, practice large discretion in the field. As time advances, the writer believes there will follow less occasion for the terms "riparians" and "appropriators" that now divide the subject.

Of course, few in the United States would contemplate that discretion should so grow as to become dictatorial power. To prevent it becoming so, it is to be hoped that a hearing in court, dedicated as the courts are to impartiality, will be preserved to complaints against abuse of the discretion. However, that is too large a subject, throughout the country, for discussion herein. The better the water administrators are posted in hydrology the fewer, one may hope, will be the abuses.

The writer would venture a final comment upon cooperation of hydrology and law. It is long-standing law everywhere that when water disturbances are made for public use, private claimants of whatever origin may be compelled to yield their water rights by conversion to a money equivalent measured by the damage suffered, what is called condemnation under power of eminent domain. Under the usual statutes, this needs only a hearing to establish the public necessity and the amount of the damage or, if such be the fact, the absence of damage—followed, of course, by payment of what damage is proved. The instances in which public use meets legal hindrance usually find the public use insisting upon proceeding with its work while leaving the question of the amount of damage, or of the absence thereof, unheard and untried. Public users sometimes feel that when they say they are doing no damage the law must take their word for it.[52] There is nothing special to water law when that attitude is disallowed; nor any hindrance to public users by whom trial of damage is willingly entertained. I have ventured to suggest elsewhere that some hydrological principles bearing upon the amount of monetary damage can be formulated.[53]

HYDE FORBES,[54] M. AM. SOC. C. E. (by letter).[54a]—The only purpose the writer has in discussing this paper is to "carry a banner" for the legal profession and courts whom the authors feel are inadequately informed and thus hold "erroneous concepts." Many of the concepts presented by the authors are not in accord with those developed by the writer through his experience and observation; but this need not be considered germane to the object of the paper which is stated to be a discussion of "the problems involved in court interpretation of the type of ground-water occurrence according to the present classification of subsurface water for legal purposes."

[52] "San Francisco's Riparian Water Case," by Samuel C. Wiel, 13 *Southern California Law Review* 204 (Univ. of Southern California, Los Angeles, Calif., January, 1940).

[53] "The Need of Unified Law for Surface and Underground Water," by Samuel C. Wiel, 2 *Southern California Law Review* 358, 1929.

[54] Cons. Engr. and Geologist, San Francisco, Calif.

[54a] Received by the Secretary February 19, 1940.

The writer has found attorneys quick to understand and appreciate geologic conditions and hydrologic phenomena and able to express these conditions in the clear and concise language necessary in all legal proceedings. Furthermore, he has found the courts quite able to follow the reasoning of expert witnesses in these matters when that reasoning was kept within the bounds of fact and not carried into the realm of, what attorneys have termed, "scientific speculation."

The opinions of courts cited by the authors apply to geologic and hydrologic conditions familiar to the writer in many cases. In such cases the opinions are most applicable to the conditions obtaining and are written in a concise manner, clear in intent and meaning. The principal item of supposed conflict between legal application and hydrologic principle, pointed out by the authors, may be one of definition. About 1923 or earlier, Mr. Meinzer canvassed the field, and those active in the field, of ground-water hydrology, in an effort to organize the facts and principles pertinent to the science, and to give the concepts involved a definition, and provide a nomenclature. This was exceedingly well accomplished and was presented during 1924.[5] The writer has found no reason to amplify or change the definition or nomenclature set up therein in presenting a great variety of ground-water conditions to the laymen or lawyers. The passage of water through interstices is the scientific meaning of the word "percolation" and is that accepted by the legal profession. Percolation is so defined in the dictionary. The legal definition applied to subsurface streams has nothing to do with the definition of percolating water in that a subsurface stream has a physical occurrence as a definite water course located beneath the earth's surface between well-defined limits or banks, through which water flows or percolates in contradistinction to a condition of no such flow or percolation through the banks.

In some western states the rule of appropriation extends to ground water moving in a water course having well-defined bed and banks confining the water within circumscribed limits. Undoubtedly that rule means just what it says and is not applicable to the body of ground water percolating widely beneath great areas of land. Otherwise the appropriator who first drilled wells could draw the water from beneath all of the lands and deprive his neighbors of a valuable property right to the benefit of distant lands or regardless of reasonable use. The Arizona case cited by the authors[20] is one in point. The authors are correct when they state that "the trial court accepted the geological evidence as conclusive" as to the occurrence of subsurface streams; but they neglect to state that the evidence accepted was that presented by one party to the action—the client of one of the authors—and that the opposition presented a mass of geologic and hydrologic evidence of the fact that the occurrence of ground water in the Salt River and Aqua Fria valleys of Arizona was of diffused percolating character. The Supreme Court of Arizona wrote its decision, parts of which are cited, after a review of all of the evidence presented by both parties. It can scarcely be claimed that either the lower or the supreme courts refused "to

[5] See especially *U. S. Geological Survey Water-Supply Papers: No. 494*, "Outline of Ground-Water Hydrology," by O. E. Meinzer, 1923; *No. 489*, "The Occurrence of Ground Water in the United States," with a discussion of principles, by O. E. Meinzer, 1923; and *No. 638-C*, "Methods of Estimating Ground-Water Supplies," by O. E. Meinzer.

[20] 39 Ariz. 65, 367, 4 Pac. (2) 369 (1931), 7 Pac. (2) 254 (1932).

accept scientific evidence indicating natural conditions"; nor that its decision "may not be well founded." It should not be said that the court established "a ruling based on its interpretation of existing geologic and hydrologic conditions." The Supreme Court studied and weighed the evidence submitted, made its findings of fact, and applied a rule of law.

The authors state (see heading "Percolating Waters"): "The most curious concept expressed in legal literature is that of 'diffused percolations * * *.'" Experience in investigating the chemical character and temperatures of ground water indicates that there is a considerable diffusion of waters that are percolating underground. Waters from many sources mingle and become diffused throughout ground-water basins. All ground water in the process of percolation is well described as "vagrant, wandering drops moved by gravity in any and every direction along the line of least resistance," when the distinction is sought to be made between widespread percolation and the subflow of a stream.

The statement, "The legal dictum that a surface stream is 'supported' by subflow is only applicable under effluent conditions," is not correct. The legal dictum, if there is one, consists of the fact that the subflow is part of the surface stream and is supplied therefrom. The subflow acts as a "support" of the surface stream in the sense that seepage of water from the surface stream is at a minimum with the maintenance of the subflow. If the subflow is removed by diversion under the water-table conditions pictured in Fig. 1, 2, or 4 the condition of Fig. 3 results with a consequent increase in seepage loss from, and a lessening of flow in, the surface stream.

In City of San Bernardino vs. City of Riverside[23] the concept of "support" cited by the authors exactly fits the physical conditions held requisite by the authors, as the streams in question were effluent streams. The citation applies to no other condition. However, in Lemm vs. Rutherford[22] the court found that the seepage from a ditch saturated the material over which the ditch flowed, and the ground-water mound so created acted as a "support" for the surface flow in that it materially reduced subsequent seepage loss therefrom. The taking of ground water from the immediate vicinity of the ditch was found to have the effect of creating an artificial draft upon the surface flow. Similar conditions attach to the other cases cited wherein artificial draft draws down the ground-water level, and in this manner "weakens and injures the natural bed."

The criticism of lawyers on the basis that "they do not generally appreciate the fact that stream flow occurs over non-saturated gravels through which water seeps from the surface to the water-table" certainly is not applicable to the lawyers met with in the writer's experience. The assertion ascribed to them would have more general application if the authors would change the words "before there can be any surface flow" to "before there can be no diminution of surface flow." As seepage from the stream builds a mound on the ground-water surface toward stream-bed elevation (as illustrated by the authors) the rate of seepage from the stream is reduced when the partly saturated capillary fringe reaches the stream bed, and seepage ceases when the ground-water elevation corresponds to stream-bed elevation. The interstitial

[23] 186 Cal. 7, 198 Pac. 784 (1921).

[22] Lemm vs. Rutherford, 76 Cal. App. 455, 245 Pac. 225 (1926).

water then acts as a "support" for the surface stream. There is nothing erroneous in the assumption that the conditions will be reversed if the ground-water elevations were to be lowered by draft upon ground water. The time affected and the measure of the effect that ground-water draft has upon surface flow is a matter that is determinable for each specific set of conditions. Some of the positive statements in that regard made by the authors toward the end of the paper are not warranted.

It is not clear what "hydraulic laws" the authors refer to in the statement ("Part I.—Occurrence and Movements of Water Underground: Effects on Surface Flow of Pumping from Influent and Effluent Subflow"): "It has been stated in legal proceedings that the ground-water level responds to pumping in the same way as the water level of a surface reservoir, which, of course, is not in agreement with hydraulic laws." Necessarily a body of ground water must be held in some container, which container corresponds to a reservoir for surface water. Any diminution in the quantity of ground water in such a basin will lower the level of the ground water over the basin. The only difference between ground-water basins and surface reservoirs in this regard is in the lag in time before a widespread effect is registered due to the frictional resistance of the materials containing the water in underground reservoirs retarding the immediate adjustment of levels.

The authors present ground-water contours as "indicative," whereas they should be referred to as "illustrative." Lawyers and the courts rightfully find it difficult to distinguish between fact and opinion when the hydrologist is not careful to make the distinction himself. It is only one step further in their confusion to assume opinion to be "unproved theory." Contour lines depicting lines of equal elevation of the ground-water surface or water table are interpolations between observations of the water surface elevation in wells or test holes scattered over an area. They are unlike ground surface contours in that they are an expression of an opinion rather than a fact. In mapping surface topography the details are presented to the eye and can be readily expressed in the interpolation made between points of determined elevation. Ground water is hidden and contour lines of its surface are necessarily the result of interpolations between measured points, expressing the judgment of the delineator based upon his experience. In the presentation of such lines to a court, one does so only as an expression of opinion based upon all determinate facts. In themselves, they are not conclusive and cannot be considered separate from the testimony and qualifications of the delineator.

The statement of the authors, "the notion that ground water must be concentrated in a restricted subsurface channel to constitute a supply sufficient for industrial or agricultural use * * * accepted by some lawyers," must have very limited application. Attorneys familiar with western water law are well informed upon the occurrence of and rights in widespread percolating water and the waters of ground-water basins. The legal decisions take cognizance of the physical facts relative to "the particular conditions existing in the area in controversy" and apply the law to establish the rights of the parties. Possibly there is room for improvement in the law applied to ground water as it has been developed in the west through a succession of court decisions. Possibly enact-

ment of legislation might provide that property rights in ground water shall be administered more effectually than through trying each case on its merits in light of the conditions unique to each locality in controversy. However, as the great variety of ground-water occurrence and conditions is hidden below the surface of the ground, differences of opinion will arise between those whose duty it is to inform the courts or administrative body upon the nature of the conditions and effects of proposed or accomplished actions if such conditions and actions are the subject of controversy.

Engineers and geologists who, through experience, have gained a knowledge of ground-water hydrology can clarify seeming misconceptions and inconsistencies if, when controversies arise, they will recommend to their clients and attorneys that they be allowed to make investigations jointly with representatives of the other party to the controversy to the end that questions of fact will be settled through stipulation. The lawyers and the courts will have no trouble in applying the law to the facts.

The legal profession and the courts use a phraseology which may not express a "concept" as it exists in the mind of the hydrologist but is clear and precise in intent and meaning to the legal and lay mind. The ground-water hydrologist should use simple words in common usage and defined in the dictionary to express his concepts in his dealings with the legal profession. The "coinage" of new terms and phrases in court testimony as well as other discussion creates confusion because the user must define and qualify each term upon each occasion of use. The difficulty that some expert witnesses have in making their ideas known to attorneys and the courts is that they do not present their reasoning in a manner that can be followed by the lay mind. All geologic and hydrologic phenomena can be presented in a simple manner, identified with ever present examples of familiar processes. The correctness of scientific reasoning can often be checked by the fact that it presents, in the words of the Supreme Court of Arizona,[20] "clear and convincing proof to the satisfaction of a reasonable man."

RONALD B. HARRIS,[55] ESQ. (by letter).[55a]—Although he is a lawyer, the writer recognizes the justification of the criticism to the fact that certain court decisions have expressed erroneous concepts of the nature of ground water.

The courts are the finders of facts which they cull from the evidence presented to them. It is the duty of the courts to distinguish between that evidence which is true and that which is false. Generally speaking, facts are presented by witnesses and where the subject matter calls for special knowledge, only those persons who can qualify as having special knowledge in that field are entitled to express opinions. If the decisions of the courts express erroneous concepts of the nature of ground water, then such decisions reflect one of two conditions: either (1) that at the time of the trial of the action the nature of ground water was not fully developed and known, except by a few pioneers in that field, and hence expressed the then deficient popular knowledge concerning the same; or (2) that the witnesses testifying in that particular case were not qualified.

[55] Harris, Willey & Harris, Attorneys at Law, Fresno, Calif.

[55a] Received by the Secretary February 19, 1940.

Of course, it is true that in all fields of learning there are charlatans who impose themselves upon the courts as experts, or, although experts, knowingly betray their profession; and their testimony, often being quite plausible, sometimes influences the courts' decisions.

However, in the matter presented in the paper, the decisions reflect the then popular concept of ground water. The need of accurate knowledge concerning ground water did not become pressing until the introduction of pumping. This did not occur until, within certain areas, surface streams were exhausted and the quest for water went underground. When pumping exceeded replenishment of the ground water, with the resultant lowering of the water table, water users affected were willing to finance the costs of investigating and determining the nature of ground water—first for the purpose of preserving its level and, second, for determining rights to its use.

The leading case in California on rights to ground water is Katz vs. Walkinshaw,[56] which applied the doctrine of correlative use to percolating water. This doctrine, of course, was already in use as between riparian users as a class. It is the writer's opinion that this doctrine is being extended and, in time, will be the universal doctrine for measuring the rights of all users of water, regardless of whether their rights arise as riparian owners, as appropriators from surface supply, or as users of ground water. In other words, all water users within a drainage area are joint users of all the waters that occur in that area and each user's rights will be correlated with each other user's rights, no matter from whence their source of supply. The necessity of making nice distinctions in classifications as to the source of supply being natural stream flow, flood water, subsurface streams, percolating water, etc., will be eliminated gradually because the measure of the user's right will be limited to that use of all the waters that are available to him, regardless of the source, which will result in the most practical and economic use of all the waters in relation to all of the users. Priorities will still be recognized but will be correlated to all other priorities in their order not necessarily as to specific sources of supply but in consideration of all sources of supply.[57]

The application of this doctrine, no doubt, will be extended to each area if, as and when, due to drought or other factors, the demand exceeds an uneconomic use of the water supply in such area. In making the greatest economic use of the waters in an area, ground water must be included.

The prevailing thought now is to prevent waters that could be put to beneficial use from escaping to the sea. The methods of accomplishing this are by stream reservoirs, by percolation basins, and by retardation by ground storage caused by irrigation. (This process is "retardation" because, although below the surface of the ground, the water is not static. It generally moves slowly in the direction of the topographical slope, and at the lower fringe of the alluvial cones or plains would seep into natural water courses and thence to the sea.)

Throughout the western states, through federal aid, millions of acres of arid lands are being brought under artificial irrigation. In doing this, great

[56] 141 Cal. 116 (1903).

[57] Peabody vs. City of Vallejo, 2 Cal. (2) 351 (1935); City of Lodi vs. East Bay Municipal Utility District, 7 Cal. (2) 316 (1936); Hillside Water Co. vs. City of Los Angeles, 10 Cal. (2) 677 (1938); and Subterranean and Percolating Waters, 190 A. L. R. 395 (1937).

changes will be made in the ground-water tables. An example of the importance of ground water in connection with irrigation is the Kings River area in California. In this area there are about 800,000 acres of land partly irrigated from Kings River. This irrigation creates a high ground-water table which is used as a supplemental supply for irrigation. About 12,000 pumping plants utilize this source of supply with a charge for electrical energy of between $1,500,000 to $2,000,000 per annum. This ground-water table becomes of paramount importance in those years when the seasonal runoff of Kings River is wholly insufficient to meet irrigation needs.

The question may well be asked, what relevancy has the foregoing to the paper under discussion? It has this relevancy: The paper demonstrated the lack of disseminated knowledge as to the nature of ground water. The trend in the law following the development of large areas of arid lands through irrigation, with its accompanying creation of new and changed water tables, requires thorough and accurate knowledge of ground water, not only in settling disputes as to the rights to use of such ground water, but in its control for the beneficial and most economical use, and in the preservation of lower lying lands from destruction by too high a water table. For this courts and water users must look to irrigation engineers. The fountainhead for their learning is the universities. Are the universities fulfilling this requirement in the education of their engineers who desire to specialize in irrigation? Are these engineers being qualified as experts in ground water? It is the understanding of the writer that, generally, they are not.

It is the writer's thought that the place to correct the misconceptions of the nature and characteristics of ground water is at the source of learning, and that is, at the universities.

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DISCUSSIONS

NORRIS DAM CONSTRUCTION CABLEWAYS

Discussion

By Gordon H. Bannerman, M. Am. Soc. C. E.

Gordon H. Bannerman,[4] M. Am. Soc. C. E. (by letter).[4a]—Congratulations are due the authors for the fine manner in which they have reported the construction of the Norris Dam cableway system. This paper has made very interesting reading, and the tests described with their results are a notable addition to the subject of modern heavy-duty cableways.

There is no question as to the desirability, and oftentimes necessity, of transmitting the horizontal thrust of the track cables to the movable tower foundations by the use of horizontal thrust wheels. This is a practical and safe method of taking care of these stresses.

Motorizing the trucks of movable towers has also greatly improved cableway designs. Another improvement has been to equip the track-cable end connections with both horizontal and vertical joints, the stresses being transmitted through eyebars and forged pin blocks. The cable tensions are taken in direct tension and shear, eliminating the undesirable **U**, or loop, bolt. This type of connection, provided with lubricating fittings, will dampen both the horizontal and vertical vibrations to a greater extent, and will relieve unequal stresses in loop bolt members that have given trouble.

It has been realized for some time that the actual stresses in operating ropes must be greater than the computed stresses, this difference in stresses being problematical. The stress values in Fig. 8, to the writer's knowledge, are the first actual stresses that have been determined in this type of installation. Similar tensometer tests have been made on the larger power-shovel operating ropes, and occasionally actual stresses were found to be 50% to 100% in excess of computed stresses.

At Shasta Dam, in California, the writer has collaborated with Francis T. Crowe, M. Am. Soc. C. E., and B. W. Goodenough, Assoc. M. Am. Soc. C. E., with the result that the button ropes on the 2,672-ft cableway spans have been increased to $\frac{7}{8}$-in. diameter and the endless ropes to $1\frac{1}{8}$-in. diameter.

Note.—This paper by R. T. Colburn, M. Am. Soc. C. E., and L. A. Schmidt, Jr., Assoc. M. Am. Soc. C. E., was published in December, 1939, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: March, 1940, by J. S. Foster, Esq.

[4] Chf. Tramway Engr., Am. Steel & Wire Co., Worcester, Mass.

[4a] Received by the Secretary March 6, 1940.

The larger button ropes on these spans will more easily support the increased number of buttons necessary, and will offer more resistance to abrasion and impact as the slack rope carriers are taken off the carriage horn by the buttons. In most instances, the endless rope is set at an erection tension higher than necessary to assure the operator that no slippage will occur on the endless drum. When this high erection tension is added to the additional tensions produced by the normal operation of the cableway and the stresses due to impact, the total tension results in a low safety factor. This fact is revealed in Table 8. A counterweighted endless rope would assure safer operating conditions, but to date has not seemed practicable.

The hoist or load line receives severe impact stresses as shown in Table 8. The hook load is raised so quickly that the wires in the strands and the strands in the rope have difficulty in adjusting themselves to this impact stress. The length of hoist line from becket to load block operates at a disadvantage with no place for adjustment to be absorbed; so this portion of the rope generally fails first. The stresses here are an accumulation of torsion stresses due to stretch. Sufficient additional length is provided so that this part of the hoist line can be cut away and a new becket hitch made.

The life of operating rope can be increased by the use of larger sheaves. At Shasta Dam, sheaves 5 ft in diameter were adopted throughout the 2,672-ft spans. The life of track cables is definitely proportional to the individual wheel loads. The twelve wheel carriages were ample for 6-cu-yd concrete loads at Norris Dam. Carriages provided with sixteen wheels are recommended for 8-cu-yd concrete loads, particularly on the longer spans, and where the track cable at the pickup point is on a steep grade.

The pickup points for the load should be as far distant as practicable from the towers in order to keep the cable angle under the individual wheels as small as possible. At this point, the load applied may be equal to the maximum load, whereas the tension in the track cable is considerably less than maximum tension, resulting in a larger angle under the load.

The cross section of the track cable shown in Fig. 2 is of a small cable. The cable, 3 in. in diameter, used at Norris Dam, contained approximately double the number of wires shown in Fig. 2. In general, track cables deteriorate due to bending stresses and not due to abrasion or tensions. For this reason, the wires must be of such size as to resist, efficiently, the repeated bending due to the rolling load.

It is unfortunate that tests were not made on the track cable with the 17-ton load at the center of the span, as this is the only point where the tension can be determined accurately. The formulas involving Fig. 10, for deflection and tension at any other point, assume a constant length on the center line of the cable, and therefore the computed tensions will always be lower than the actual tensions.

AMERICAN SOCIETY OF CIVIL ENGINEERS

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DISCUSSIONS

STANDARDS OF PROFESSIONAL RELATIONS AND CONDUCT

Discussion

By Messrs. Charles F. Scott, M. J. Evans, R. L. Sackett, Alonzo J. Hammond, A. B. McDaniel, C. B. Burdick, John M. Hayes, and G. W. Howard

Charles F. Scott,[13] Esq. (by letter).[13a]—From Table 1 in Professor Mead's paper it appears that only 25% of the ideal equipment of the engineer is technical knowledge and skills whereas 75% concerns character and judgment and other personal qualities. Everyone should read the list and study it. The knowledge of fundamentals and much of the technique required in practice are included in the college curriculum; it is what is prescribed and taught to the undergraduate student. However, even a perfect scholastic grade in them does not insure a successful engineering career. The other 75% are the personal qualities, the things which cannot be forced into the student, but which he must develop within himself.

In his thorough analysis Professor Mead presents ten different headings, "Codes" or "rules" with an average of a dozen items under each. In a preliminary paragraph he refers to these items as "the following principles" and his repeated reference to Item (26) in his bibliography declares in effect that the rules are simply applications of the Golden Rule. The student may do well to take to heart the early items specifically addressed to him, and regard the remainder as an example of the difficulty of expressing intangible principles by rules. There is a professional attitude, a professional spirit which is the essence of a true profession. The scores of rules are indications of the principles as applied to particular cases; the student should use them to aid him in understanding the professional spirit and cultivating the professional attitude.

M. J. Evans,[14] Esq. (by letter).[14a]—Character building and personality development have been taken entirely too much for granted in the engineering

Note.—This paper by Daniel W. Mead, Past-President and Hon. M. Am. Soc. C. E., was published in January, 1940, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: March, 1940, by Messrs. Louis E. Ayres, Ivan C. Crawford, Walter H. Wheeler, Charles R. Gow, J. T. L. McNew, and W. L. Waters.

[13] Prof. Emeritus of Electrical Eng., Yale Univ., New Haven, Conn.

[13a] Received by the Secretary February 9, 1940.

[14] Evans Associates, Inc., Chicago, Ill.

[14a] Received by the Secretary February 13, 1940.

profession. The fundamentals involved are admitted by all thinking persons, but the emphasis has been lacking.

The most critical problem facing industry today is a definite shortage in the supply of men capable of filling the bigger positions, particularly those earning more than $10,000 per yr. This shortage is due, not to a lack of ability, but to a lack of understanding of the simple fundamentals of what might be termed, for lack of a better phrase, "Human Engineering." In the final analysis the problem that faces the successful engineering executive is that of dealing with people, and of securing their hearty cooperation in the accomplishment of any objective.

It is generally admitted that at least 75% of a man's success is due to these intangibles, and yet the profession has given them little systematic thought and study. If this had been done, the tremendous economic waste of the five years 1935–1940, that has arisen through strikes and lack of understanding between management and labor would have been eliminated. The engineer has brought mechanical operations to a high degree of efficiency; but he has not carried his share of the load in the matter of increasing the human understanding in the plants of the United States. To quote Dean Dexter S. Kimball of Cornell University, "Our wisdom has not kept pace with our knowledge."

In numerous contacts with young engineering students in American universities during the five years since 1935, the writer has been amazed at the complete ignorance shown by many young men of the principles which the author has so forcefully expressed in his paper. Not that they have never heard of these principles—these vital fundamentals of life have seldom been stressed as an essential part of the engineering profession.

It would seem that one of the most effective ways in which this can be done is through the sharing, on the part of senior engineers, of their broad experience and understanding. Younger men are very anxious in most cases to do the right thing; but they cannot be expected to find the most advantageous road if those who have gone before and who have been successful do not prepare the necessary road maps.

R. L. SACKETT,[15] M. AM. SOC. C. E. (by letter).[15a]—The statement by Professor Mead on the ethics of engineering is very complete and most admirable. In addition to reviewing the relations of engineers with each other, with clients, and with the public, he has emphasized the importance of personal qualities on the future advancement and usefulness of the engineer. In 1915, when the first investigation of engineering education was made by Professor Mann[4] for the Carnegie Foundation for the Advancement of Teaching, some 5,000 engineers focused attention on the greater importance of character, judgment, accuracy, industry, the ability to work with others and to direct them, as compared with technical knowledge. The "Code of Courtesy and Personal Conduct" formulated by Professor Mead is a very thoughtful and thorough

[15] Dean Emeritus of Eng., The Pennsylvania State Coll., State College, Pa.

[15a] Received by the Secretary February 16, 1940.

[4] Address on "Engineering Education," by C. R. Mann, *Proceedings*, Am. Soc. C. E., February, 1916, p. 98.

digest of principles which each engineer, young or old, ought to make cardinal virtues.

Much is said today about the importance of social science as a subject that should be included in the education of all engineers because of the influence of engineering works on human welfare and behavior. This code of personal conduct is of more importance as the foundation for good social relations than are most of the textbooks on social science. Human relations begin with the human being and his personal balance, his restraint, poise, good humor, outlook, attitude toward others, his sense of responsibility, his devotion to the profession, and his realization of his debt to society. The technical training of engineering graduates is adequate. Its quality has been attested by a careful survey[16] conducted by the Engineers' Council for Professional Development.

The colleges have always been aware of the importance of character, and have attempted to protect and develop it. There is now evidence of an awakening to a sense of responsibility for guidance toward personal attributes and technical competence which together characterize a profession. Combined, they constitute a chapter in social advance which marks the engineering profession as qualified for public recognition. Education should contribute toward personal development and this emphasis by Professor Mead will encourage engineering schools in further efforts to assist young engineers in realizing professional status.

ALONZO J. HAMMOND,[17] PAST-PRESIDENT, AM. SOC. C. E. (by letter).[17a]—Both professional relations or principles of ethics and code of conduct or practice are combined in this paper. The code of practice adopted by the Society, January 18, 1927, states (34)[17b] in its "Preamble": "Any Code of Ethics is founded on the Golden Rule," so the author in his "Introduction" has developed his theory of the philosophy of life which, for the young man and student, forms an exceedingly valuable part of the paper, and is worth the most thorough study.

Engineers are faced with the query, "Is engineering a profession?" The answer in some instances has been a question mark, based on the assumption that the Society has not acquired, nor do the membership in general subscribe to, an established and agreed "Code of Ethics." Critics state that engineers have not advanced to the stage of the so-called three learned professions—theology, law, and medicine—due to this lack of an established and adopted code.

Every one, no doubt, will subscribe to the need of the brief "Code of Ethics" embodied in the seven paragraphs printed in the Society Year Book (29); but has the Society attempted to instill into the mind of the student and young engineer a philosophy of life that will lead him to follow, naturally, the precepts in that brief code? The great importance of this paper is the presentation of principles applicable to all professional positions and its emphasis to

[16] Seventh Annual Report, E.C.P.D., October, 1939, p. 6 *et seq.*

[17] Cons. Engr., Chicago, Ill.

[17a] Received by the Secretary February 16, 1940.

[17b] Numerals in parentheses, thus: (34), refer to corresponding numbers in the Bibliography of the Appendix of the paper, and at the end of this discussion.

the young man. Of greater importance to the engineering profession, as such, is the opportunity thus afforded to take advantage of the study as basic material; to cull from it a complete code of what may be termed "Principles of Engineering Ethics" or "Canons of Professional Ethics" (whichever heading meets the fancy); and to set up a code or set of rules to be approved by the members which will satisfy the requirements of a professional spirit.

Referring briefly to the paper to indicate what the writer would include in such a code:

As a preamble he would make the following statement:

"As Engineering is the science of controlling the forces, and utilizing the materials of nature for the benefit of man, and the art of organizing and directing human activities in connection therewith, the practice of Engineering is a Profession."

Following such a preamble, the writer would make a part of it the paragraph of the paper entitled "Requirements of the Profession." Then following and becoming part of the code with such mention as may be deemed desirable, the following sections would be included: "III. Public Relations of the Engineer," "IV. The Personal Relations of the Engineer," "V. The Engineer's Relations to Client or Employer," "VI. The Engineer's Relations with His Employees," and "VII. Relations of the Engineer with the Contractor."

The substance of these five sections, together with the suggested preamble, would form the long-desired and long-needed "Principles of Engineering Ethics" —a complete code for the engineering profession.

In his deeply interesting Romanes lecture, Prof. Thomas H. Huxley has stated the opinion "that the Ethical progress of Society depends upon our combating the cosmic process which we call the Struggle for Existence." Since, as he adds, "we inherit the cosmic nature which is the outcome of millions of years of severe training, it follows that the 'Ethical Nature' may count upon having to reckon with a tenacious and powerful enemy as long as the world lasts."

May the combat at this time be successful in the adoption and application of the "Principles of Engineering Ethics."

(34) "Code of Practice," Manual of Engineering Practice No. 1, Am. Soc. C. E., p. 1.

A. B. McDaniel,[18] M. Am. Soc. C. E. (by letter).[18a]—A notable contribution has been made by Professor Mead to the literature on professional engineering relations. His scholarly statement is worthy of careful study by all practicing engineers and especially by the younger members of the profession.

From observation, both within and without the scholastic field, the writer has been impressed with two outstanding tendencies of engineering education:

1. To cram the student with large doses of technical knowledge; and
2. To stress the importance of material success.

[18] Cons. Engr., Washington, D. C.

[18a] Received by the Secretary February 20, 1940.

Most engineers whom the writer has questioned have stated that knowledge required in their work has been acquired largely from their experience since graduation. Should not the engineering schools concentrate their resources and efforts on mental training—the development of those qualities referred to in Items Nos. 1 to 4, inclusive, of Table 1 entitled "Characteristics Necessary to Success in Engineering"?

Many engineers go through life without a clear understanding of how to live. This fundamental should be taught to engineering students, beginning with the freshman year, through a comprehensive course in human and engineering relations. Such a course should be given by a practicing engineer with a long and varied experience and the ability to inspire his hearers.

How few people, and especially engineers among professional men, realize the importance of contacts and the maintenance of friendly relations with all sorts and conditions of people! One of the writer's classmates owed his start in life and education to the kindly services he rendered to an elderly man during the latter's illness on a lake boat trip between Buffalo, N. Y., and Detroit, Mich. The sick man, John D. Rockefeller, Sr., was so impressed with the unsolicited and helpful services that subsequently he aided the engineer in becoming one of the leading industrial executives of his time.

Too much emphasis cannot be placed on the engineer's duties and responsibilities as a citizen of the community in which he lives. He should become an active member of his local religious, civic, and professional organizations and should utilize every opportunity to make contacts, participate in meetings, and serve on committees. He should represent his profession with dignity, efficiency, and integrity on all occasions.

Over generations of students, the tradition has been to do just enough work "to get by." Fortunately, this practice is not so prevalent in engineering schools as among other schools of college grade. The writer remembers an acquaintance who boasted that he received his baccalaureate degree from a well-known college without even opening a textbook; he majored in the "cramming" courses of a large and flourishing private school in the vicinity of the campus. The student should be encouraged to do more than the assigned lesson, so that later in life he will have acquired the habit of going beyond his immediate task.

C. B. BURDICK,[19] M. AM. SOC. C. E. (by letter).[19a]—The biologists teach that man made the greatest stride in advance when he rose up on his hind legs and thus freed his hands to grasp things. Since that far off day, in common with all organic life, he has exploited the animal and vegetable kingdoms, so far as he could, to satisfy his wants. Also in common with the higher forms of animal life the growing brain very soon developed the value of a deference to its kind. Thus customs were formed; then came the common law, and the statutory law. These things came about because they were deemed of value. Civilization is only possible because of them.

Thus it would seem proper that the engineer, gaining his livelihood in a specialized calling, should have a light to guide him especially applicable to

[19] Cons. Engr. (Alvord, Burdick & Howson), Chicago, Ill.

[19a] Received by the Secretary February 26, 1940.

that calling. Like a sign upon the road, it should be useful to others who travel that way. The Society is fortunate that the author has taken the opportunity to formulate a road map of professional relations and conduct. That the subject is important is well shown by Table 1, in which it is the weighted judgment of 5,441 engineers that knowledge and technique are rated at only 25% of the qualities necessary for success in engineering. That the subject is timely is indicated by the probably provable fact that 99% of all published matter relates to knowledge and technique.

The qualities that promote success might all be typified under four heads—character, ability, personality, and knowledge. If equal values are assigned to each, one corroborates the concensus of opinion previously stated that knowledge, although indispensable to success, is overshadowed by qualities subject to cultivation that are given sparse attention in most engineering literature. In this matter it must be conceded that opportunity has an important bearing upon the degree of success. However, inasmuch as success is a goal not subject to exact definition, and must be considered in the light of opportunity, as a rule opportunity can be ignored except by continual striving to make use of such opportunity as may be afforded.

The qualities tending toward success can best be utilized if there are "rules of the game." A lasting success can only be attained if there is a reasonable adherence to such rules. Such adherence promotes the general welfare, and it benefits the individual who practices it. No doubt it is true that virtue is its own reward. There is a satisfaction in it. There is no lasting satisfaction without it. Apart from this fact, however, it pays dividends to the individual to adhere to the rules of conduct. Benjamin Franklin expressed this thought in a homely way when he said—"if the rascals knew the advantages of virtue they would become honest men out of rascality."

This paper is timely. The views expressed are sound. They are based upon a wealth of experience and thoughtful consideration. In principal they are applicable to most forms of human endeavor; in detail they apply to the specialized calling of engineering in all its branches. The Society would do well to give such matters more attention. In the engineer's quest for knowledge let him not forget to cultivate the qualities that make it useful, and thus promote the best interests of the profession.

JOHN M. HAYES,[20] JUN. AM. SOC. C. E. (by letter).[20a]—The sincere thanks of every member of the profession are due the author for his able presentation of such an important topic. Standards of professional relations and conduct should be of vital interest to every engineer—young and old.

The Society has great need for an unofficial code of ethics which touches upon every activity in the engineer's life—one that is personal with which to supplement the present formal Code of Ethics of the Society. This paper should be studied thoroughly by every engineer and every engineering student. It will give faith and guidance to the young and reassurance to the old.

[20] Junior Structural Engr., TVA, Highway and Railroad Div., Chattanooga, Tenn.

[20a] Received by the Secretary March 4, 1940.

The author's treatment of the subject of opportunity is especially encouraging. Many people seem to believe in an adage that "opportunity knocks but once and returns no more." This is a pessimistic attitude. Some think that "pull" and influence have more to do with success than anything else. It is stimulating to have an authority emphasize the importance of sound character and hard work as factors influencing success. The quicker one learns to expect advancement only after he has earned it by hard work the happier he will be, both in his professional and private life. The advice to make effective use of spare time should be heeded by every young engineer, as should also the advice, "The young engineer should not be a 'yes' man."

Much is heard now of obtaining more public respect for the engineer and the profession. The profession will gather respect when each individual member of the profession lives so as to command this respect. There could be no better way for an individual to command respect than to live by the principles stated in the paper. In order for one to practice these principles in his daily life, he must be constantly on the alert, because it is easy to forget good intentions. The author has previously written:[21] "The persistent exercise of the higher ideals, as a basis of life and action—which is often of even greater importance than a supreme sacrifice to the continued development of civilization—requires continued and unremittent effort which human nature, in the main, is apparently too weak to sustain." Here he puts emphasis upon "unremittent effort."

It should be the duty of every member of the Society to read and reread the paper from time to time. Thus, he will keep these principles, which should govern his actions, constantly in mind and eventually their practice will become a habit with him. One must realize that it is not often possible to live up to his ideals completely; but one should never lose sight of his goal. Every one makes mistakes, but if he keeps striving he will finally improve himself. The faithful and conscientious practice of these standards of professional relations and conduct in one's daily life will not only make one a better engineer but a happier man.

G. W. HOWARD,[22] JUN. AM. SOC. C. E. (by letter).[22a]—Since this is an age of transition and instability, it is well worth while for every man to pause and consider what principles he lives by. At a time when the beliefs and practices of centuries are being questioned, and in many instances discarded entirely, it is incumbent upon the professional man to examine the status of both himself and his profession in order to preserve his equilibrium. If the title "engineer" is to continue in its rightful place in the professional world, it is necessary for engineers to be governed by the precepts enunciated in this paper. It is impossible for the engineer to bury his mistakes with the ease of the doctor; nor can his errors be rectified by a later decision of a jury or judge, as in the case of the lawyer. On the contrary, the engineer must assume sole responsibility for the

[21] "The Engineer and His Code," by Daniel W. Mead, Past-President and Hon. M. Am. Soc. C. E., *Transactions*, Am. Soc. C. E., Vol. 101 (1936), p. 1483.

[22] Associate Engr., Office, Chief of Engrs., Washington, D. C.

[22a] Received by the Secretary March 4, 1940.

success or failure of an undertaking. It is doubly important, therefore, that he understand the situation on adopting the profession.

In general, it seems that confusion may arise either from the lack of condensation of the paper, or from the failure of the author to include an adequate summary at the conclusion. Not only ideas, but at times the sentences themselves are repeated almost verbatim. The repetition of identical material may leave the reader with an indefinite conception. The presentation would be clearer and just as adequate if one general set of codified principles, applicable to all phases of the profession, were used, with the addition of specific codes embodying features pertaining only to one particular phase.

However one may regard the organization of the paper, it is undoubtedly true that the ideas contained therein are indeed worthy. As stated by the author, the ethical principles presented in the various codes are axiomatic. The qualities that make for success in any profession are essentially the same. The "yardstick" of attainment has always been one's native intelligence, character, knowledge, and capacity for hard work, each dependent upon the other. The innate ability of a man is wasted when it is not joined to integrity and industry; and the inverse is true.

On the other hand, correct evaluation of the qualities of success by fitting each into the narrow category of the percentage basis, it seems, is impossible. The individual differences existing between men are too great for the formation of any set system. Success is intangible and may not be analyzed as may a mineral deposit—so much gold, so much silver, copper, or gangue mineral. If it were possible, however, to apply a table of characteristics to an individual, there would still be cause for questioning of Table 1 entitled "Characteristics Necessary to Success in Engineering." The author states ("Introduction: Ability to Apply Principles to Practice"): "The important matter is the great weight given to characteristics on which dependability and integrity rest as compared with those factors common to educational requirements; and yet those latter factors are absolutely essential to the success of the engineer." The table in question, in its milder form, gives 75% of the credit of success to "characteristics on which dependability and integrity rest," and only 25% to those of technical knowledge. This proportion is believed to be in error. When a man is ill, he may be concerned about his doctor's character, it is true; but how much more concerned is the same man about his doctor's technical skill and knowledge! Likewise, when an important project is under construction, it is hoped that the engineer in charge has the proper "perspective of life," but it is demanded that he have thorough technical qualifications. Moreover, if the percentages of Table 1 are to be accepted, the futility of the present engineering educational program must be admitted, and the engineering student should substitute courses in "How to Win Friends and Influence People" for those involving technique.

The importance of hard work and steady application is emphasized by the author. Since the Garden of Eden, the fallacy has been prevalent that work is a curse. As a matter of fact, the greatest blessing ever to reach mankind is work, it seems. Even for the man of average ability, no art or science is believed too difficult for action to circumscribe. In the words of the ancient

proverb: "Work conquers all things." Those great men of the past who have accomplished most had one quality in common—the capacity for hard work. Otherwise, Michelangelo would never have covered the walls of the Sistine Chapel; nor would Galileo have developed the Copernican theory of the solar system; nor would Newton have discovered the differential calculus.

Yet as Stevenson[23] says: "Idleness so-called, which does not consist of doing nothing, but in doing a great deal not recognized in the dogmatic formularies of the ruling class, has as good a right to state its position as industry itself." Leisure time, and its proper use, are in a sense as important as work itself. If a man is to have a well-integrated personality, he should have hours free from his profession in which to develop his individual preference for books, painting, music, or whatever his interest. It seems that as a man grows older and his physical powers decline, he should find added enjoyment and content through the proper use of his mental faculties, as he has learned to use them in his youth. This matter has been covered well by Robins Fleming.[24]

Another characteristic of the successful engineer, which was mentioned but not emphasized, is that of self-reliance. The contemporary psychological theories of self-expression are not subscribed to, but the writer agrees with Emerson,[25] who has said: "The highest merit we ascribe to Moses, Plato, and Milton is, that they set at naught books and traditions, and spoke not what men but what they thought * * *. Whoso would be a man must be a non-conformist * * *. It is as easy for the strong man to be strong, as it is for the weak to be weak." However much one likes or dislikes himself, he must make the most of what he has, and stand upon his own feet.

The implication is not intended that the proper sense of humility should be lacking. Every man who will trouble himself to view the light of Pythagoras, Socrates, Confucius, da Vinci, and others, cannot help losing his smugness and inflated ego. Furthermore, it seems that one of the greatest advantages it is possible to enjoy is the opportunity of working under men of wide experience. Here one may learn from the actual practical experience of others and thus avoid many of the pitfalls of theory or the trial-and-error method.

[23] "An Apology for Idlers," by Robert Louis Stevenson.
[24] *Civil Engineering*, February, 1940, p. 77.
[25] "Self-Reliance," by Ralph Waldo Emerson.

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DISCUSSIONS

PRESSURE-MOMENTUM THEORY APPLIED TO THE BROAD-CRESTED WEIR

Discussion

By I. M. Nelidov, Assoc. M. Am. Soc. C. E.

I. M. Nelidov,[21] Assoc. M. Am. Soc. C. E. (by letter).[21a]—The principle of impulse-momentum has been applied by the authors to the flow over a broad-crested weir, and their theoretical results are represented in the formula common to weirs:

$$Q = C\,L\,H^{1.5} \qquad (28)$$

Experiments were also conducted by the authors, and their results, together with the results of experiments made by others, were applied to interpret the theoretical derivations.

The principal conclusion advanced in the paper is that the ratio of depth to head on the weir is $K' = \frac{1}{K} = 0.5$, which permits one to compute the coefficient of discharge C. The latter coefficient varied from 2.25 to 2.70 for Bazin's experiments and from 2.68 to 2.65 in Cornell experiments. The authors did not give their values of the coefficient C. These, computed by the writer from Table 1, vary from 2.50 to 2.87.

The writer wishes to discuss the connection between the specific case chosen by the author and the application of the momentum principle to more general cases of flow over the broad-crested weir of any cross section, with any shape of approach channel, the latter varying from the cross section of the weir to that of the reservoir. Consider a flow, as shown in Fig. 11, over the broad-crested weir at section 3–3 from a channel at section 1–1.

The forces resulting from water pressure and acting on the flow are shown shaded. In the profile in Fig. 11(*a*) they represent the hydrostatic pressures on sections 1–1 and 3–3 and the reactions of the invert of the transition. In the plan (Fig. 11(*b*)) they represent the projections of the hydrostatic pressures on sections 1–1 and 3–3, and of the reactions of the sides of the transition.

Note.—This paper by H. A. Doeringsfeld, Esq., and C. L. Barker, Assoc. M. Am. Soc. C. E., was published in December, 1939, *Proceedings*. Discussion on this paper has appeared in *Proceedings*, as follows: March, 1940, by Messrs. J. C. Stevens, and H. G. Wilm.

[21] Senior Engr. of Hydr. Structure Design, State Div. of Water Resources, Sacramento, Calif.

[21a] Received by the Secretary February 20, 1940.

The hydrostatic pressure P_1 acts in section 1–1, based on depth H above the crest of the weir and its width b_3. At section 3–3 the hydrostatic pressure P_3 acts, in the opposite direction, based on depth d_3 on the weir and its width b_3. Below the level of the weir, and on its sides, the additional hydrostatic pressure in section 1–1 is counteracted by the horizontal projections of the reactions of the invert represented in profile and in plan by dotted lines. The remainder of these pressures, directed downstream, is represented by the force P_0. The force P_0 is a function of d_4, $H - d_3$, and $b_1 - b_3$. Because of the curvature of

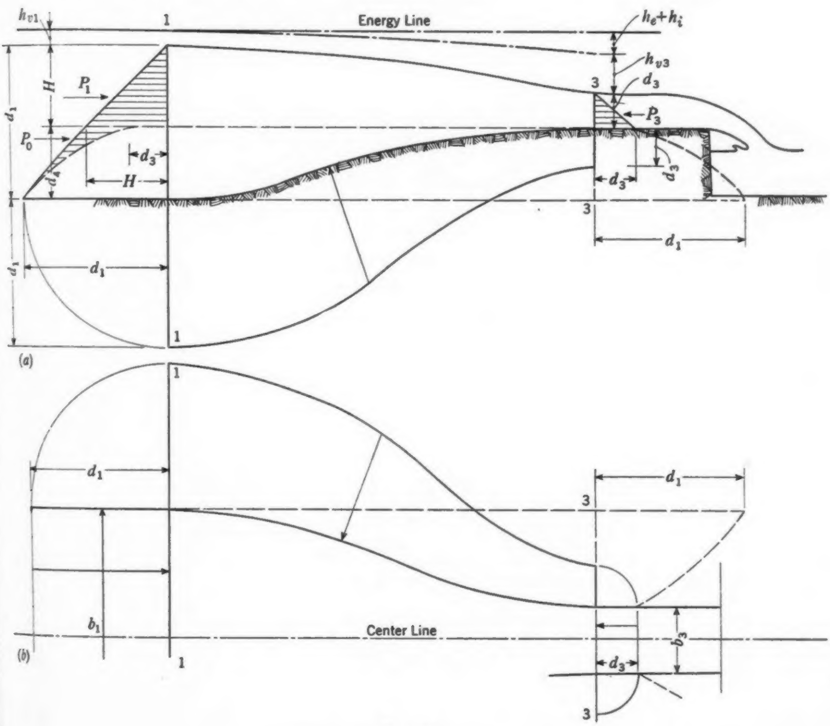

FIG. 11

the water surface between sections 1–1 and 3–3, neither the reactions of the invert nor the force P_0 is composed of linear unit pressures. On the contrary, these pressures vary curvilinearly with depth. This factor reduces the pressure P_0 considerably.

In the case of a rectangular channel of constant width it will be equal to:

$$P_0 = w\, C_0\, b\, (d_1 - H) \left(\frac{H - d_3}{2}\right) \dots\dots\dots\dots\dots (29)$$

in which C_0 is a coefficient depending on the shape of the invert, and is smaller than unity. The impulse momentum equation for a general case may be

written as follows:

$$(P_0 + P_1 + 2\,A_1\,h_{v1}) - (P_3 + 2\,A_3\,h_{v3}) = 0 \quad \text{(30)}$$

in which P_0 is the force due to excess of hydrostatic pressures in the downstream direction, acting below the level of the weir, and on its sides; P_1 is the force due to hydrostatic pressures in section 1–1 above the level of the weir, based on width of weir b_3; P_3 is the force due to hydrostatic pressures in section 3–3; A_1 is the cross-sectional area at section 1–1 based on depth d_1; A_3 is the cross-sectional area at section 3–3, based on depth d_3; h_{v1} is the velocity head at section 1–1; and h_{v3} is the velocity head at section 3–3.

For a case of flow from a reservoir, $h_{v1} = 0$, and Equation (29) becomes:

$$(P_0 + P_1) - (P_3 + 2\,A_3\,h_{v3}) = 0 \quad \text{(31)}$$

In general, the problem contains three unknowns: P_0, Q, and d_3. If P_0 and Q are known, d_3 can be found from either Equation (30) or Equation (31). In case only P_0 is known, the problem can be solved only for the flow from a reservoir. In this case the flow over the broad-crested weir will be with the critical depth, which renders h_{v3} a known portion of d_3; and d_3 can be determined from Equation (31). A relationship between h_{v3} and d_3 obtained from the theory of critical flow is:

$$h_{v3} = n\,d_3 \quad \text{(32)}$$

The values of n are as follows:

Channel section	Value of n
Rectangular	0.500
Parabolic	0.333
Triangular	0.250
Trapezoidal	Equation (33)

For the trapezoidal section in the foregoing,

$$n = \frac{1 - \dfrac{m\,d_c}{B_c}}{2} \quad \text{(33)}$$

in which: m is the side slope of the channel referred to a horizontal unity; d_c is the critical depth; and B_c is the critical top width. If the losses are desired, they can be computed from the equation of specific energy:

$$h_{v1} + H = d_3 + h_{v3} + h_e + h_i \quad \text{(34)}$$

in which h_e is the entrance loss, and h_i is the impact loss. Returning to a case of a rectangular channel of uniform width, with vertical upstream face of the weir discussed by the authors: $P_0 = 0$, and Equation (30) may be rewritten as follows:

$$\frac{H^2}{2} + 2\,d_1\,h_{v1} = \frac{d_3^2}{2} + 2\,d_3\,h_{v3} \quad \text{(35)}$$

For the flow from a rectangular reservoir, $h_{v1} = 0$, and Equation (35)

becomes:

$$\frac{H^2}{2} = \frac{d_3^2}{2} + 2\, d_3\, h_{v3} \qquad (36)$$

Since, for this case, $h_{v3} = n\, d_3 = 0.5\, d_3$, by Equation (31), after substituting in Equation (36):

$$d_3 = \sqrt{\tfrac{1}{3}}\, H = 0.577\, H \qquad (37)$$

It may be noted that for an ideal flow without losses, $d_3 = 0.667\, H$.

The losses of entrance and impact corresponding to $d_3 = 0.577\, H$ will be, by Equation (34), $h_e + h_i = H - 0.577\, H - 0.288\, H = 0.135\, H = 0.468\, h_{v3}$, or 47% of the velocity head of velocity on the weir. In the case discussed by the authors, due to velocity of approach, the depth d_3 varied between 0.51 H and 0.55 H and losses varied between 0.30 h_{v3} and 0.07 h_{v3}.

A practical problem arises of determining the discharge from a reservoir of irregular shape. In this case the simplest procedure is to estimate the impact and entrance loss as a percentage of h_{v3}, depending on entrance conditions, and solve for d_3 from Equation (34). The discharge then may be computed either by the formula, $Q = A_3\, V_3$, or by the weir formula, $Q = C\, A\, \sqrt{H}$, which becomes $Q = C\, L\, H^{1.5}$ for a rectangular weir. The value of C may be found by Equation (18), or by the expression:

$$C = (\tfrac{2}{3})^{1.5} \frac{\sqrt{g}}{\left(1 + \frac{p}{3}\right)^{1.5}} \qquad (38)$$

in which

$$p = \frac{h_e + h_i}{h_{v3}} \qquad (39)$$

is the ratio of loss in terms of velocity head and velocity on the weir. Equation (38) is obtained from the expression

$$C = \frac{A_3\, V_3}{L\, H^{1.5}} \qquad (40)$$

after substitution of $A_3 = d_3\, L$ and $V_3 = \sqrt{2\, g\, h_{v3}}$, with $h_{v3} = \frac{d_3}{2}$ and d_3 expressed through H and p from Equation (34). The force P_0 can be computed then from Equation (31) if desired and compared with actual existing conditions.

The value of P_0 is maximum for zero losses and becomes zero for the maximum loss obtainable for the given shape of the reservoir and weir. For the rectangular reservoir, the ratio of $\frac{P_0}{P_1}$, determined from Equations (31) and (34), is equal to 0.333 for zero loss and is equal to 0.000 for a loss ratio $p = 0.468$. For a sharp-crested weir the ratio $\frac{P_0}{P_1}$ was found by Charles W. Harris,[22] M. Am.

[22] "An Analysis of the Weir Coefficient for Suppressed Weir 5," by Charles W. Harris, *Bulletin No. 22*, Univ. of Washington, November 15, 1923.

Soc. C. E., to be equal to 0.222. For a reservoir of irregular shape the ratio $\frac{P_0}{P_1}$ will change, but the trend of its variation will remain the same.

In closing, the writer wishes to remark that the generally adopted use of the coefficient of discharge C, without proper correlation with the energy conditions of the flow, does not help to explain the mechanics of the latter. For instance, what does coefficient $C = 2.85$ mean if it is not related through definite entrance and impact losses to the physical condition of the entire entrance and not just to the "entrance lip"? This is exactly what has been done in the past—the coefficients C were assumed arbitrarily, based only on the shape of the "entrance lip" and for a rectangular weir, whereas, in practice, weirs and approach channels are encountered in any form. The work done by the authors will enable designers to correlate the losses occurring in transition to the coefficient of discharge C in a more rational manner.

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